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The Natural Sciences and the Social Sciences

Some Critical and Historical Perspectives

Edited by I. Bernard Cohen

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THE NATURAL SCIENCES AND THE SOCIAL SCIENCES

Some Critical and Historical Perspectives

Edited by

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FOREWORD – "AN INNOVATION IN THE HISTORY OF SCIENCE"

It is a privilege to be able to comment briefly here with the purpose of setting the stage for this remarkable innovative volume in the history of science. The action on the stage itself is masterfully carried out by Professor Cohen (serving both as stage director and as actor himself) and his several colleagues. Their performances, their individual essays, speak brilliantly for themselves. I can only add some remarks on the historical and intellectual background and context of this original enterprise, which I predict will be appreciated not only for its own substance but as a set of models for future work in the history of the sciences, natural and social.

Senior historians of science will remember from their own experience, and more recent members of the profession will probably have heard of the battles or war that prevailed in the 1950's and 1960's between the so-called "internalist" and "externalist" schools in their field. Perhaps over-simply put, the "internalist" historian of science felt that his (very few hers then) task was to trace the development of the substantive *idea or conceptual* systems of science as independent elements, wholly on their own terms, untouched by other social or cultural factors. Little attention was paid even to the importance of what is now intensively studied, the organizational and leadership arrangements for the advancement and maintenance of science. Often, of course, in their focus on the ideas and concepts of science, "internalists" included what would be called philosophical and religious ideas. Such certainly was the case for the classic and very influential work of Alexander Koyre.[1] But the "internalists" were chiefly oriented against what they saw as crude, Marxist emphasis on the economic and social influences on science in the work of such writers as the Russian, Boris Hessen,[2] and the Englishmen, Bernal[3] and Hogben.[4] This defensiveness ignored work which transcended the "internalist" – "externalist" dichotomy, work such as Robert Merton's *Science, Technology and Society in Seventeenth Century England*[5] and my own *Science and The Social Order.*[6] These works proceeded on the assumption of a complex societal system in which science, both natural and social, was only one element, however

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important; there were many other equally important social structural and cultural elements, including economic and political factors on the social side, and religion, values, and ideology on the cultural side.

The present volume is another definitive sign of what has been the case on the whole in the history of science for the last twenty years or so, that the "internalist" – "externalist" difference has been dissolved, not only theoretically but by a mass of valuable work proceeding on the new assumption, that science has reciprocal relations with all the other components of society. Read in the light of this assumption, the present volume is not only about the reciprocal relations of the natural and social sciences but also about the close involvement of both of them with many other social and cultural factors. Perhaps this is only what George Sarton, the founder of the history of science and of its journal (in 1912), *ISIS*, had in mind when he said, in introducing the new journal, that it was to be " . . . the sociological journal of the scientists and the scientific journal of the sociologists".[7]

Another, albeit implicit, virtue of the present volume is that it eschews all simplistic determinisms. Not only is neither natural science nor social science the simple determinant of the other, but neither, also, is simplistically and always determined by any other single or set of social or cultural factors. Science as a whole is partly independent of other factors in the social system, partly interdependent with them.[8] So it is also for natural science and social science *vis-à-vis* one another. And so is it also for all social structural and cultural factors in the social system.[9] This is not to say that the task of determining the multiple and complex interrelationships of the natural and social sciences with one another and with a variety of other social and cultural factors will be an easy one. But it can be done, as the several essays in this volume bear witness. In science, even if the necessary tasks are difficult, they must be done. Easiness has no inherent virtue. We need a whole set of case studies, like the present one, to begin to establish how and how much science is independent, how and how much it is interdependent with the rest of the the social system.

The achievement of these tasks in the history of the interrelations of the natural and social sciences will often require collaboration among experts from the different fields. Too often, natural scientists have had simplistic and limited knowledge of the social sciences, and *vice versa* for the social scientists. In some cases, a single individual from one side has gone to school to the other and managed a result that is satisfactory to all. But more often, pair or even team collaboration will be necessary to get it right.

My impression is that such collaboration will be easier at the present for the historians of the natural sciences, who have a solid tradition of archival and collaborative research. There is very little of such a tradition and such performance among the social scientists.[10] The tradition will have to be built up and legitimated, the performance will have to be demonstrated, before the social scientists can claim a place of equality with the historians of the natural sciences. To see what their future should be like, social scientists should pay as much attention to this volume as natural scientists.

Some efforts toward that legitimation of a proper history of the social sciences have recently occurred. The American Sociological Association, for example, has appointed an Archives Project Committee, of which I am the Chair.[11] Our initial purpose is to draw up a guide to all the archival materials for sociology and sociologists that are now scattered all over North America. (P.A. Sorokin's papers, despite his long tenure at Harvard, for example, are not at Harvard, but at Calgary.) If the history of sociology is to be good history it will have to be based on such archival materials, as well, of course, as on oral histories.

I have been waiting for a long time for a volume like this one. I hope others will welcome it as much I do.

Columbia University BERNARD BARBER

NOTES

1 *From the Closed World to the Infinite Universe* (Baltimore: Johns Hopkins University Press, 1957).

2 Boris Hessen: "The Social and Economic Roots of Newton's 'Principia,'" in *Science at the Cross Roads* (London: Kniga, 1931).

3 J.D. Bernal: *The Social Function of Science* (New York: Macmillan, 1939).

4 Lancelot Hogben: *Science for the Citizen* (London: G. Allen & Unwin, 1938).

5 Bruges: OSIRIS, IV, Part 2, 1938. For a recent collection of critiques and appreciations of this book, see I. Bernard Cohen, ed., *Puritanism and the Rise of Modern Science: The Merton Thesis* (New Brunswick, N.J.: Rutgers University Press, 1990).

6 Glencoe, Ill.: The Free Press, 1952. For an extension of this work, see B. Barber: *Social Studies of Science* (New Brunswick, N.J.: Transaction Books, 1990).

7 Reported in May Sarton: *I Knew A Phoenix: Sketches for an Autobiography* (New York: W.W. Norton, 1969), p. 69.

8 For a powerful argument for the partial independence of the ideas and concepts of

science against some recent interesting relativist arguments for the determination of these ideas by social and cultural "interests," never too well defined, see Stephen Cole: *Making Science: Between Nature and Society* (Cambridge: Harvard University Press, 1992).

[9] For a generalized statement of this theoretical assumption and of a provisional model for the societal social system, see Bernard Barber: *Constructing The Social System* (New Brunswick, N.J.: Transaction Books, 1993).

[10] For two recent exception, see Charles Camic (ed.): *Talcott Parsons: The Early Essays* (Combridge: Harvard University Press, 1992); and Bruce C. Wearne: *The Theory and Scholarship of Talcott Parsons to 1951*. (Cambridge: Cambridge University Press, 1989). Older exceptions can be found in the continuing work of George W. Stocking on the history of anthropology.

[11] Attention should be called to a growing body of important studies by historians, social scientists, and historians of science that deal with various aspects of the history of the social sciences and the interactions of the social sciences and the natural sciences. Many of these works are mentioned in the Preface to this volume and in the references in the individual chapters.

PREFACE

The present volume focuses on certain historical interactions between the social sciences and the natural sciences.[1] While there is a large body of literature on the logical, philosophical, and "scientific" foundations of social science in general and of individual social sciences, such literature generally has not been conceived in a historical mode. The result is that, with some notable exceptions, it tends to examine the methods of the social sciences by comparison and contrast with the methods of the natural sciences but does not to attempt a critical analysis of the historical encounters and interactions between social scientists and the natural sciences of their day.

There is also a rapidly growing literature concerning the history of the individual social sciences, and a major journal in this area, *Journal of the History of the Behavioral Sciences*, ably edited by Barbara Ross, is currently in its twenty-ninth volume. Yet most of the research and writing on the history of the social sciences, however valuable in its own terms, has tended to be either internal to the discipline or related to the larger intellectual and social matrix and has not been specifically oriented to the concurrent developments in the natural sciences. Two very useful compendia, for example, Pitirim Sorokin's *Contemporary Sociological Theories* and Joseph Schumpeter's *History of Economic Analysis*, barely mention the natural sciences. This lack is glaring in Sorokin's analysis of the nineteenth-century organismic sociologists who drew heavily on such current or then-recent developments in biology as the cell theory, the discoveries concerning embryological development in mammals, the physiology of the "milieu intérieur," and the germ theory of disease; this feature is also conspicuous in Schumpeter's presentation of the founders of marginalist economics who based their concepts and methods on those of rational mechanics. An extreme example of this lacuna is the important and useful historical analysis by Werner Stark: *The Fundamental Forms of Social Thought* (London: Routledge & Kegan Paul, 1962), containing many lengthy quotations that deal with advances in the biological sciences (e.g., the work of Rudolf Virchow); it has no discussion of these biological principles, no hint of their importance in

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, xi–xxiv.

the development of the natural sciences nor of their significance as examples of interactions between the natural sciences and the social sciences; similarly, the lengthy extracts and descriptions of the use of physical science by social scientists are presented without any inquiry into their having been used as other than pure rhetoric. Even so insightful and important a contribution to knowledge as Dorothy Ross's recent *The Origins of American Social Thought* (Cambridge/New York: Cambridge University Press, 1991) takes no real cognizance of the actual physical and biological sciences that were used by the social scientists whose careers she explores. For example, although Henry Carey (see Chapter 1, §1.5 below) insisted that his social system was an extension of the system of the physical universe, and that he had found a social equivalent of Newton's law of gravity, this part of his work is mentioned only in passing in a single sentence; although Irving Fisher stated in unambiguous terms that he was basing his economics on rational mechanics (see Chapter 1, §5 below), there is not even a mention of any technical aspect of the science used so extensively by this pupil of J. Willard Gibbs. These authors are not here cited for criticism; their works had very different purposes than to explore the interactions between the social sciences and the natural sciences. But they do indicate in a dramatic way that there is another important dimension to the history of the social sciences, a need to understand by case histories how the social sciences and the natural sciences have interacted in the centuries since the advent of "science" as we know it today.

There are some scholars, however, who in recent years have begun to study the history of the social sciences, taking cognizance of the interactions with natural sciences; their writings have proved to be of notable value for the investigations presented here (notably Chapter 1). In particular I have drawn heavily on the writings of some historians of science: Theodore Porter, Robert Richards, Judith Schlanger, George Stocking, and Norton Wise.[2] A group of economists have been studying the foundations of their subject – in particular, neoclassical or marginalist economics – in the physical sciences and also the biological sciences; those whose writings have proved most important in the context of the present volume include Philip Mirowski, Roy Weintraub, Neil de Marchi, Claude Mesnard, Vernard Foley, Margaret Schabas, and Arjo Klamer.[3] Additionally, the fairly recent studies on statistics – notably by Ian Hacking, Stephen Stigler, Lorraine Daston, William Coleman, Gerd Gigerenzer *et al.*, and by Lorenz Krüger and the Bielefeld study group[4] – have given new perspectives to the relationship of core techniques to social problems and social theory in this important subject. Although the present work does not deal with anthropology, notice must be taken of the important new historical work in this area, primarily the serial publication, founded and edited by George Stocking, called *History of Anthropology*, of which volume 7 (1992) is the most recent.[5]

The present volume was conceived to illustrate by case histories the actual ways in which the natural and social sciences have interacted. It will be noted that three chapters are devoted to an aspect of this relation that is usually overlooked: the ways in which the natural sciences have been influenced by the social sciences. Some writers who have been aware of this kind of interaction have cited Darwin's use of Malthus in formulating his theory of evolution based on natural selection. Some others have been aware that Virchow, the founder of the great medico-physiological revolution associated with "cellular pathology," frequently used the concept of the state and of social organization in formulating his scientific concepts.[6] But it may come as a surprise, especially to physicists who do not believe the social sciences to be of any use to their own discipline, to discover in our Chapter Eleven (by Theodore Porter) that mathematical physics (in the persons of James Clerk Maxwell and Ludwig von Boltzmann) was indebted to sociology.

Because of the nature of the subject and the difficulty in finding qualified authors for the several parts, there are important examples of the interactions between the natural and the social sciences that are only barely mentioned or not discussed at all in the present volume. Furthermore, there has been no attempt to introduce material from each of the several social sciences; for example, psychology and anthropology are not discussed, nor is history, while political science appears primarily (in Chapter 4) in the setting of the Scientific Revolution of the seventeenth-century. An additional limitation is the exclusion of general proposals or philosophical statements concerning the state of the social sciences or their future, consideration being strictly limited to actual attempts to create or improve a social science. Thus purely methodological writings, such as those of John Stuart Mill, are not generally explored in the chapters of this book.

My own study of the interactions of the social sciences and the natural sciences was originally undertaken as an extension of previous research on scientific creativity, which had focused on the different ways in which the sciences have influenced one another. It was but a short step to extend this enquiry into the parallel phenomenon of the interaction of the natural sciences with the social sciences. When I first undertook this investigation, I naively believed that the vast and steadily accumulating literature of books, monographs, and journal articles on historical aspects of the social sciences would provide a useful and readily available, if not fully digested, body of reliable secondary source material to serve my purpose. The very existence of two multi-volume encyclopedias of the social sciences, replete with biographies and bibliographies and historical expositions of main themes, seemed a guarantee that – except in rare cases – I should not have to do all the spade-work research in primary sources that is almost always required in my own field of history of science. After all, I reasoned, the social sciences represent a proud ancient profession with a direct lineage that could be traced to Plato and Aristotle. Surely social scientists would have been concerned with the interactions of their disciplines with the natural sciences during the centuries since the Science Revolution![7]

I was aware, furthermore, that some social sciences (notably psychology, political science, economics, and sociology) regularly included courses in the history of their respective disciplines in their programs and that others (notably political science, sociology, economics, and history) made creative use of texts of past great masters in their teaching and research. So it seemed to me that my study of the interactions of the natural and the social sciences could take advantage of the fact that the social sciences are unlike the natural sciences in the way that they make use of their history as part of professional training and that they draw upon the writings of the past as part of the useful literature of their subjects. Even economists, the most like physicists of the social scientists, are usually familiar with such fine points of their history as the difference between the systems of Adam Smith and Ricardo, the distinction between the ideas of Jevons and Walras, or the relation of Menger and the Austrian school to Marshall. Few physicists would have an equally sound and extensive knowledge of the work of their nineteenth- and early twentiety-century predecessors.

Another factor that led me to suppose that my task would be easier than in fact it turned out to be was the constant litany in the different social sciences – primarily economics and sociology – of their status as true sciences. I quite naturally fell into the error of believing that, in their studies of the past, social scientists would have particularly stressed the different ways in which their illustrious predecessors had made use of the science of their respective eras – drawing both inspiration and useful analogies from the work of their contemporary natural scientists as well as from philosophers and their fellow social scientists.

No sooner had I started my research, however, than I quickly discovered that I was mistaken on all the above counts. There was precious little literature, if any, that took account of the ways in which social scientists of the past three centuries had interacted with their fellow natural scientists or had attempted to use concepts, principles, theories, or methods of the natural sciences at large. Additionally, the reverse interaction – the influence of social sciences on the development of the natural sciences – was all but completely ignored and in some cases even denied.

I did not understand how this situation could possibly exist until I happened to re-read Robert Merton's Introduction to the collection of his essays on *Social Theory and Social Structure* (New York: The Free Press, 1968, earlier editions, 1957, 1949). In the course of this general prolegomenon, the important distinction is made between "the *history* of sociological theory" and "the *systematics* of certain theories with which sociologists now provisionally work." This confusion of genuine historical investigation and the search for "utilizable sociological theory" of the past invades much of the writing on the history of sociology and also the other social sciences. A paradigmatic example is given in a work to which I have already referred, Pitirim Sorokin's retrospective survey, *Contemporary Sociological Theories*, a useful first guide, especially for Russian source materials not easily available elsewhere. The title is somewhat misleading, since this work comprises historical surveys of different varieties of sociological theories, usually beginning with the seventeenth century or earlier. The stated main purpose is to provide background information on the current state of knowledge through analytical and critical summaries of the ideas of nineteenth-century and early twentieth-century pioneers. Sorokin's aim was not to understand the thought of the past so much as to criticize the writings of all previous ages from a "presentist" standpoint and to seek for any useful principles which may be still valid in today's systematics. As such this work, however useful as a preliminary survey, is more a contribution to practical sociological studies than a truly historical enquiry and must accordingly be used with the greatest caution.

Merton's analysis applies equally well to other social sciences. Much of the historical writing on the history of economics is conceived in relation to economic theory, as a subject of direct use in understanding or in teaching economics. This field thus tends to be dominated by a critical attitude that has come to be known as whiggism in history: an attempt to judge the ideas of the past by present standards rather than to explore such ideas on their own. This aspect may be seen in the fact that many of the works in this area are devoted to specialized topics of current interest today rather than to the nature of the subject as it existed in some past age. There are, or course, important exceptions – of which an example is Schumpeter's *History of Economic Analysis*, referred to earlier, a highly personal statement drawing on a tremendous store of first-hand knowledge and deep historical insight. One of the most interesting general histories of any of the social sciences, this great work sparkles with individual judgments based on the author's prejudice and the state of economics at the time of writing.

From a long historical point of view, the influence of the natural sciences on the social sciences is not a new phenomenon born of the Scientific Revolution, but rather appears to be as old as the idea of science itself. In his "Politics" ($1290^{b}21$–$1291^{b}13$), Aristotle recommended that the study of constitutions of states and the determining of "the forms of government" be modeled on the methods of classifying "the different species of animals." According to Sir David Ross (*Oxford Classical Dictionary*, p. 116, §9), Aristotle even attempted to "achieve for States" the same "precise description of their types as he gives for animals in the *Historia Animalium*."

In the Middle Ages and the Renaissance the idea developed of the body politic, in which the functions of government were explained by analogy with human anatomy and Galenic physiology. One survival, of many, from this physiological political theory is the concept of a "head" of state. In the seventeenth century (as explained in Chapter 4) the discoveries of Harvey and the influence of Descartes altered this concept to its more modern form, with which we are familiar today. Another science that was related to political theory is astronomy. In the Renaissance, Elizabeth's power was displayed in a diagram modeled on the current astronomical diagrams of the system of celestial spheres. Elizabeth I (reigning in the "sphaera civitatis") became the prime mover of the system, with inner spheres representing her virtues or "planetary" attributes: abundance, eloquence, clemency, religion, fortitude, prudence, and majesty. The Scientific Revolution produced a modified astro-political diagram in which Louis XIV was presented in a background of a Copernican rather than an Aristotelian system of the universe, set in a system of Cartesian vortices and marked with the date of birth for computing the royal horoscope. Louis's designation as "roi soleil" may be compared to Harvey's analogy (presented in Chapter 4, §1) between the role of King Charles and the function of the heart. There was, clearly, a long-standing tradition of associating theories of the state or social organization with the current conceptions of science. Our volume, however, deals only with the three centuries following the Scientific Revolution, with the specific ways in which the social sciences have interacted with developing modern sciences.

The present volume was conceived because social scientists, with their own professional agendas, have not fully explored the ways in which the ideas, laws, principles, or theories of their fields have developed by making use of or interacting with the physical and biological sciences or mathematics. The question must arise of whether social scientists have thereby left out of their considerations one of the primary well-springs of the thought of the past. The present volume attempts an answer in a display of example after example of the special impact of ideas from the natural sciences on the development of the social sciences. Such examples will indicate the nature of this transfer of ideas and, at the same time, show why standard historical works on the development of the social sciences must be constantly supplemented by and monitored by an examination of the primary documents of the past.

I have shown (in Chapter 4) that Hugo Grotius was a great admirer of Galileo and conceived his celebrated treatise on international law to have been written in the spirit and manner of a work on geometry. This aspect of his work, of great significance in the present context, is not even mentioned (nor even alluded to) in the article on Grotius in either the older *Encyclopaedia of the Social Sciences* (1932) or the more recent *International Encyclopedia of the Social Sciences* (1968). A recent reprint of an English version of Grotius's treatise omits altogether the preface in which he explicitly states that his work was conceived on a model of classical geometry, even though it does not display the formal aspects of theorems and deductions in the Euclidean mode, as it the case for Spinoza's *Ethics*. Grotius's ideal of geometry is relevant to an evaluation of his work because this feature determined that he would deal with abstract cases rather than historical examples or examples from the disputes of his own age – an aspect of his presentation for which he has been roundly criticized.

The situation is somewhat the same for another example (also discussed in Chapter 4), James Harrington's politico-social thought, expressed in his *Oceana* and other writings. Harrington's ideas assumed significant proportions in the eighteenth century, influencing many of the American Founding Fathers and becoming embodied in the American Constitution. Although Harrington expressly founded or justified his system on the basis of the new Harveyan physiology, there is no mention of Harvey or his science in the *Encyclopaedia of the Social Sciences*; in the *International Encyclopedia of the Social Sciences*, Harvey's influence is mentioned in passing, but not in a way that would give the reader any sense of the possible extent of Harvey's actual influence on Harrington.

An equally striking example of the neglect of the study of the interactions between the natural sciences or mathematics and the social sciences is provided by an early essay of Leibniz (also discussed in Chapter 4). Although Leibniz devoted this essay to a mathematical demonstration of a method of selecting a king for Poland, this does not appear to have merited any notice whatever in standard presentations of the history of political thought. This essay is not even mentioned in a recent volume devoted to Leibniz's political writings.

Even when the scientific component of social thought is introduced, the significance may be lost because of a lack of understanding of the science of the past. An example (discussed in Chapter 1) involves Berkeley's conception of a social analogue of the Newtonian gravitational cosmology. Berkeley's presentation shows that he understood perfectly the principles of Newtonian celestial dynamics, explaining planetary orbital motion as a combination of a continual central accelerating force and an undiminished initial component of linear inertial motion along a tangent. In the presentation of Berkeley's Newtonian sociology, in Sorokin's textbook survey, Berkeley's correct physics is reduced to the incorrect form of a "balance" between centripetal and centrifugal forces, a standard elementary textbook error that has long plagued the teaching of physics. Berkeley's sound Newtonian physics is reduced to utter nonsense by the additional statement by Sorokin that stability occurs when the alleged centrifugal force is less than the centripetal force. Berkeley certainly would have known, as Sorokin evidently did not, that in such a hypothesized example the unbalanced centripetal force would not produce stability but rather instability, with a resulting inward motion toward the sun or other center of force. A somewhat similar example (analyzed in Chapter 1) is Henry Carey's model of a social analogue of Newton's gravitational physics, mentioned or discussed in almost every historical work on social theories that I have encountered. In not one have I found a recognition that Newton's law of universal gravity, the basis of Carey's social science, is stated incorrectly by Carey, not once but several times.

A considerable literature exists on the organismic sociologists of the late nineteenth and early twentieth centuries, a company that includes Otto Bluntschli, Paul von Lilienfeld, Albert Schäffle, Herbert Spencer, Lester Ward, Corrado Gini, Walter Bradford Cannon, A. Lawrence Lowell (president of Harvard), and Theodore Roosevelt (President of the United States). With the exception of Spencer, all of these figures are discussed in historical surveys or works on sociological theory without any reference to their use of the leading biological and medical theories of their times. This absence is all the more remarkable to the degree that some of these organismic sociologists (notably Lilienfeld, Schäffle, and Cannon) included extensive bio-medical tutorials in their sociological presentations. Thus (as shown in Chapter 1), however extravagant the ideas of these organismic sociologists may seem to us today, our judgment should take account of the relation of their sociological ideas to the main currents of contemporaneous biological and medical thought.

One aspect of the interactions between the natural and the social sciences that is all but wholly absent from the literature of both the history of the social science and the history of the natural sciences is the possible influence of the social sciences on the rise of the biological and physical sciences. Accordingly, there is a special value to the three chapters comprising Part IV of the present book. I have mentioned that Darwin is known to have been influenced by Malthus's ideas concerning population growth while formulating his concept of natural selection. S. S. Schweber (in Chapter 9) has summarized his findings on the sources of Darwin's ideas, notably the influence of the current ideas of agronomy on Darwin's thinking. Camille Limoges (in Chapter 10) has traced the history and use of another idea which Darwin obtained from the social sciences and which became of particular importance in the nineteenth century in the context of the cell theory. The division of labor gained prominence through the writings of Adam Smith, although the idea had been put forth earlier by such writers as William Petty and Benjamin

Franklin. This concept, as we learn from Limoges's presentation, was particularly significant in the thought of the French biologist Henri Milne-Edwards, who used it in relation to the role of individual cells in the physiology of the organism and from whom it was transmitted to Emile Durkheim, who wrote his major doctoral dissertation on the sociological division of labor. Theodore Porter (in Chapter 11), as I have mentioned earlier, has traced the direct and acknowledged effect of the work of Adolphe Quetelet on the physics of both Maxwell and Boltzmann.

The general importance of Quetelet and the rise of statistical thinking in the social sciences may be seen as a special case of the interaction of quantitative considerations and mathematical techniques and social thought. Ian Hacking (in Chapter 2) has traced the development and use of numerical social data by concentrating attention on the enumeration of cases of suicide during the nineteenth century and the ultimate use of these numbers by Durkheim. A parallel study by Bernard Lecuyer (in Chapter 3) explores the significance of quantitative and probabilistic or statistical thinking in nineteenth-century social thought, illuminating the ways in which Quetelet's influence was related to the general rise of probabilistic thinking in the first half of the nineteenth century. We are reminded that the statistical point of view aroused considerable alarm and that many thinkers – e.g., John Stuart Mill and Auguste Comte – considered statistics the resort of incomplete and faulty science which had failed to produce a simple Newtonian one-to-one relation between cause and effect. Comte not only pilloried Quetelet and others for adopting a statistical point of view but even gave up his original title of "social physics" because it had been used in a probabilistic framework by Quetelet; this was the occasion of his invention of the name "sociology." The subsequent development of social thought may be seen to a considerable degree as a tension between the ideas of Comte and Quetelet, between a social science exhibiting simple cause and effect and one based on statistical considerations – a tension that has not completely disappeared.

In many ways the heart of the present volume is the set of five historical essays comprising Part III. Chapter 4, on the first encounters during the Scientific Revolution, explores the ways in which the new ideas of Galileo, Descartes, and Harvey and the ideals of mathematics, combined with the science of motion, directly influenced the social sciences produced by Grotius, Vauban, Spinoza, Leibniz, Hobbes, and

Harrington. The mathematical spirit of the age is exhibited in the geometric form of presentation by Leibniz and Spinoza, the mathematical abstractions of Grotius, and the demands for social numbers or censuses by Vauban, and the ways in which Graunt and Petty sought to apply a new form of mathematics developed by businessmen (commercial arithmetic) to problems of polity.

Although Newton's ideals proved valuable for social scientists such as Malthus (as explored in Chapter 1, §4), there has never been a social model built directly on either Newtonian rational mechanics or the Newtonian system of the world. But those who dealt with social science continually introduced examples from Newtonian physics. One such case history is explored by Noel Swerdlow (in Chapter 5), in which Sir William Blackstone introduced Newtonian principles in the surprising and wholly unexpected setting of a legal decision. This event may be contrasted with the example of Stanley Jevons, explored by Margaret Schabas (in Chapter 6), in which Newtonian rational mechanics combined with post-Newtonian supplements such as d'Alembert's principle served to justify, by way of analogy, the introduction into dynamics of a system of differential equations. This was a stage in the development of neoclassical or marginalist economics on the foundation, by means of analogy, of rational mechanics (including such post-Newtonian principles as those of d'Alembert and Hamilton) plus energy physics.

One of the influential developments within economics was the system of Karl Marx, with its insistence on a labor theory of value. Much has been written about Marx and Darwin and attention has frequently been called to Marx's expressed admiration for Darwin and his gift to Darwin of an inscribed copy of *Das Kapital*. It is not always noted, however, that this was an afterthought on the part of Marx, since the inscribed copy (preserved in Darwin's library in Down House) is the second edition of 1872 rather than the original edition of 1867. We may be especially grateful to Giuliano Pancaldi for clarifying (in Chapter 7) the intellectual relations between these two titans of the mid-nineteenth century. In particular, Pancaldi has documented and explained both the rise and fall of Marx's admiration of Darwin and the eventual replacement of Darwin in Marx's pantheon by an obscure popularizer of science named Trémoux.

A major theme of the present volume is the role of analogies in the development of the social sciences. The use of analogies provides an important perspective for understanding the thought of Herbert Spencer, as developed by Victor Hilts (in Chapter 8). A useful distinction may be made between analogies and homologies and between both of these and metaphor, in particular (as in Chapter 1) to call attention to problems that are likely to arise in using concepts, laws, or theories from the natural sciences in the social sciences. In the nineteenth century there were two notable developments in the use of the natural sciences as sources of analogies for the social sciences. One was the above-mentioned development (explored in Chapter 1, §5) of a mathematical marginalist economics by such figures as Jevons, Walras, and Pareto on the model of rational mechanics plus energy physics; the other (explored in Chapter 1, §6) the use of the cell theory and certain allied aspects of biology and medicine by the organismic physiologists.

Anyone who studies the relationships between the natural and the social sciences quickly becomes aware that this is not a purely academic topic but rather one that has close links to policy questions in a number of different major ways. First of all, the social sciences carry a measure of legitimation by the degree to which they resemble the natural sciences and actually incorporate features, concepts, laws, or theories of the natural sciences. Because most people think of physics when they consider what a science should be like, social sciences are most impressive to the general public when they are based on extensive numerical foundations or exhibit mathematical considerations. A social science that shows the effects of interaction with the exact sciences will be more effective as an instrument for public policy than one which seems to be centered on questions of ethics or social philosophy. Additionally, any public support of the social sciences under the umbrella of "science," as in the case of the National Science Foundation, will seem most appropriate – and may therefore more readily become fact – for those parts of social science that most show the effects of interaction with or emulation of the most advanced natural sciences. Such questions are directly related to the images that natural scientists have of the social sciences and were of notable significance (as described in Chapter 1, §1) during the Congressional hearings on the establishment of the National Science Foundation.

In recent decades, there has been considerable concern expressed by natural scientists for the present state and future needs of the social sciences. This broad subject is relevant to the main assignment of the present volume, although it is far too complex to be incorporated into a single chapter. Accordingly, a different kind of presentation was envisioned, based on an extended series of focused interviews conducted by the editor with Harvey Brooks, following the lines developed in the graduate seminar on Science, Technology, and Public Policy which was conducted for many years at the John F. Kennedy School of Government by the editor, Harvey Brooks, and Don K. Price. In this way we have been able to use the personal experience, knowledge, and insights of Prof. Brooks, based on his long-term service in the area of national policy, including membership in the President's Scientific Advisory Committee (PSAC), the National Science Board, the Committee on Science and Public Policy (COSPUP) of the National Academy of Science and its successor. This format allowed me to draw on and to record Prof. Brooks's very important initiatives in activities for the promotion of the social sciences in a way that would not have been possible in a chapter of his own composition.

The research on which this book is based has been generously supported by the Richard A. Lounsbery Foundation. I am especially mindful of the courteous consideration and continued kindness of the Director, Mr. Alan McHenry, whose warm support and friendly encouragement has been a helpful factor in the course of my own research and, in particular, in bringing this volume to conclusion. As always, I have a deep gratitude to Julia Budenz, who has worked through many drafts of my own chapters – each of which stubbornly tried to achieve book-length proportions with each successive revision. I am also thankful that I have been able to call upon Prof. Elaine Storella of Framingham State College (Massachusetts) for research help and for continued assistance in revising and checking my several versions. Stuart Strickland was of great assistance in criticizing the early drafts of all chapters. The research assistance and computer skills of Katharine Downes have been very important in the completion of this volume.

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NOTES

[1] In this Preface, as in the volume presented here, reference is made to social science and social scientist (or social sciences and social scientists) in early periods before such terms were in current usage. On this topic see the *Note on Social Science & Natural Science*, following this Preface, where reasons are given for using the terms natural science and natural scientist.

[2] The studies by Porter and Wise deal primarily with the interactions of physics (and mathematics) with economics in the nineteenth century; Porter has also been exploring some of the aspects of numeracy and quantification in social science at large. Richards has been analyzing certain aspects of nineteenth-century social theory, primarily in America and Britain, in its general intellectual-cultural and social background, tracing its roots in the contemporaneous sciences. Judith Schlanger has examined the role of metaphor in organismic theories at large. Stocking has been reorganizing the history of anthropology, showing – inter alia – its contacts with the other social sciences and with certain main aspects of the natural sciences.

[3] Many of their works are cited in various parts of Chapter 1.

[4] These works are referred to in nn. 28, 30, 36, ch. 1 infra.

[5] I do not take account here of the growing literature on the history of anthropology and psychology, since the case histories in the present volume do not come from either of these fields. In this regard, however, it should be noted that anthropology has had a long tradition of writing its history and that psychology has long been known for having produced a large body of distinguished historical writing, for which see the *Journal of the History of the Behavioral Sciences*.

Furthermore, since the case histories from political science are drawn only from the seventeenth century, I take no account here of the vast body of writings on almost all phases of the history of this subject. For similar reasons, I have not discussed the literature concerning history and science.

[6] See Ch.1, §6 infra.

[7] Although there are few general works on the interactions of the natural and the social sciences, there are many important monographs or articles on particular aspects of this general topic. Many of these are cited in footnotes throughout this volume. Some examples, to which particular attention may be called, are Paul Lazarsfeld: "Notes on the History of Quantification in Sociology," *Isis*, 1961, *52*: 277–333; Bernard Lecuyer & Anthony R. Oberschall: "The Early History of Social Research," *International Encyclopedia of the Social Sciences*, vol. 15 (1968), pp. 36–53; A.R. Oberschall (ed.): *The Establishment of Empirical Sociology* (New York: Harper and Row, 1972); and the brief but incisive presentation by Theodore Porter: "Natural Science and Social Theory," pp. 1024–1043 of R.C. Olby, G.N. Cantor, J.R.R. Christie, & M.J.S. Hodge (eds.): *Companion to the History of Modern Science* (London/New York: Routledge, 1990).

Special note should be taken of the important study by M. Norton Wise (with the collaboration of Crosbie Smith) on "Political Economy and Natural Philosophy in Nineteenth Century Britain," four parts, *History of Science*, 1989–1990, vols. 27, 28.

A NOTE ON "SOCIAL SCIENCE" AND ON "NATURAL SCIENCE"

Throughout certain parts of this book, the terms "natural science" and "social science" (or "natural sciences" and "social sciences") are used to designate, respectively, the physical and biological (and earth) sciences plus mathematics and the subjects known today as social or behavioral sciences.[1] Roughly speaking, these divisions correspond to the German "Naturwissenschaften" and "Sozialwissenschaften"[2] and are in current use in the Anglo-American world. The use of these two terms – natural sciences and social sciences – when dealing with any chronological period before the mid-nineteenth century is somewhat anachronistic to the degree that it imposes on earlier thought the rigid categories and values of a later time. Today the phrase "science of society" would suggest a subject much like physics or biology but in the eighteenth century and well into the nineteenth the implication would have been only a system of organized knowledge. When Thomas B. Macauley wrote that "Politics is an experimental sciences," he meant no more than that this subject was a system of organized knowledge that was based on experience, the same sense in which these words "experimental" and "science" had been used by Hume and Burke (see Chapter 1, §1.1). Such examples alert us to the dangers of using such terms as "science" or "experimental" anachronously.

In many places in this volume (the Preface, Chapters 1 and 4, Chapter 12) the physical and biological sciences are referred to as "natural sciences," a term that may embrace mathematics. In an earlier presentation of my researches into the interactions of the natural sciences and the social sciences – at a meeting convened by Karl Deutsch and John Platt at the Wissenschaftszentrum in Berlin in 1982 – I introduced the dichotomy of "mathematics and the natural and exact sciences" and the "social sciences," but for convenience of discourse I abbreviated "mathematics and the natural and exact sciences" into the simpler expression "sciences."[3] In the first comment on my paper, Alex Inkeles criticized this usage. I had "obviously," he said, implied a difference in values assigned to the two fields of creative endeavor, one being "science" – "natural" and "exact" – the other "social." The justice of

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, xxv–xxxvi.

his criticism has led me to use the term "natural science" (and its plural "natural sciences") in order to avoid any pejorative implications, even though there may be some possible ambiguity because "natural science" may wrongly suggest "natural history" or the life sciences. I have long believed, however, that if one were seeking an antonym for "natural" science, it would not be "social" science but rather "unnatural" science; which, in turn, suggests that the proper anytonym for "social" science would be "anti-social" science.

The designation "social science" arose and became current in the late eighteenth century. The introduction of "social science" has two somewhat distinct aspects. First of all there is the actual occurrence of the term; second, the emergence of a concept in which knowledge of society is perceived to be a "science" in the sense of the physical and biological sciences. A good part of this book is devoted to an exploration of the ways in which what we would call the social sciences made use of the established natural sciences, beginning with the age of the Scientific Revolution (see Chapter 4). Many examples show the different ways in which a variety of thinkers, under whatever name or rubric they classified their activity, conceived their own subject in relation to the natural sciences and mathematics of their day. Therefore, for expository purposes I may have somewhat anachronously used the term "social sciences" (and also "moral sciences") for their thoughts and writings on such topics as political theory or statecraft, organization of the state or of society, natural law, international law, economics, and kindred subjects.

I do not know who first used the terms "social science" and "science of society." In a letter to John Jebb, written from London on 10 September 1785, the American statesman John Adams (later to become the second president of the United States) wrote of "the social science." A year before, in a letter to A.M. Cérisier, he applauded the way in which French savants (Cérisier among them) had "turned to the subject of government"; he voiced his judgment that "the science of society is much behind other arts and sciences, trades and manufactures." Even earlier, in June of 1782, Adams had declared that "politics are the divine science."[4]

I do not believe that Adams invented these expressions. In those days, however, as has been mentioned, the term "science" did not have the identical meaning which it was to acquire later in the nineteenth century. The nearest equivalent of what we would consider to be a science, in the sense of a natural science, was natural philosophy, but that subject was more akin to our physics plus astronomy and part of chemistry. (See, on this topic, Chapter 1, §1.1.)

The earliest recorded use in print of the actual expression "social science" ("science sociale") seems – according to Keith Baker – to have been in 1781 in a pamphlet addressed to Condorcet.[5] It has been suggested that since the term "art sociale" was commonly used by the Physiocrats before the Revolution, perhaps the transformation to "science sociale" occurred before 1791.[6] In any event, Condorcet himself used the new term in a draft plan presented to the Committee on Public Instruction of the Legislative Assembly in January 1792. Condorcet also introduced "social science" in his writings after 1792, notably in his "Esquisse,"[7] translated under the title *Outlines of an Historical View of the Progress of the Human Mind* (London, 1795). Faced with a new and difficult expression, the British translator chose to render "science sociale" as "moral science,"[8] a name used widely in England throughout the nineteenth century for social science.[9] In France the equivalent, "sciences morales," was in common usage early in the nineteenth century, as in the name of a "class" in the Institut de France, constituted after the Revolution: Sciences Morales et Politiques.

"Social science" entered American English in a translation of Destutt de Tracy's *Treatise on Political Economy* (Georgetown, [Washington] D.C., 1817), sponsored by Thomas Jefferson, to whom Destutt had sent the manuscript, which he could not then publish in France. Jefferson apparently checked the translation and wrote a prospectus approving the use of a number of neologisms, among them "social science."[10] In British English, "social science" seems to have come into being through a circuitous route that included a Spanish translation, made by Toribio Nuñez (Salamanca, 1820), of some selections from the writings of Jeremy Bentham. Nuñez introduced "ciencia social" into the title: *Espíritu de Bentham: Sistéma de la ciencia social*. Bentham later congratulated Nuñez for his use of "ciencia social," referring to "the science so aptly styled by you the *social science*."[11]

The history of this development has been admirably encapsulated by Victor Branford as follows:

> Between Vico's 'New Science' and Comte's 'Sociology' the infiltration of various kindred phrases, such as Social Science, Science of Society (Condorcet), Science of Man (St. Simon), would seem to mark a general tendency toward the expansion of science into the field of humanistic studies. Among Comte's contemporaries, J.S. Mill (only eight years

younger than Comte) held pronouncedly that the time was ripe for marking off from other studies – both scientific and philosophical – a general social science, and for this he himself proposed a particular designation. In 1836 Mill defined the scope and character of this department of studies, using as titular synonyms, these, among others phrases – Social Philosophy, Social Science, Natural History of Society, Speculative Politics, and Social Economy. This essay of Mill ('On the Definition and Method of Political Economy') appeared six years before the completion of the 'Positive Philosophy.' Lacking the large historical interests of Comte, Mill necessarily conceived of Social Science in a considerably different way from Comte. But after the appearance of the 'Positive Philosophy,' Mill was very considerably modified in his views of Social Science.[12]

The use of "moral sciences" became quite extensive during the nineteenth century in England. Thus in John Stuart Mill's *A System of Logic, Ratiocinative and Inductive* (London, 1843), Book Six on "The Logic of the Moral Sciences" discusses the methodology suitable for the social sciences. But in the text itself, Mill uses both "sociology" and "the social science" as distinct from political science or political economy or history. In the beginning portion of Chapter Nine, Mill originally wrote in his manuscript about "the Social Science . . . which I shall henceforth, with M. Comte, designate by the more compact term Sociology." On reflection, however, he would not so easily pass over this neologism, based on the compounding of a Latin and a Greek root, and so the published version discusses "the Social science . . . which, by a convenient barbarism, has been termed Sociology."[13] By the end of the nineteenth century moral sciences had become the name used in Cambridge University and elsewhere for the subject now known as philosophy.

In French culture the expression "sciences morales," which had been in regular use since early in the nineteenth century, has become obsolete. Curiously enough, it has been said – by Etiemble, the quixotic defender of the purity of the French language – that the factor causing a change from "sciences morales" to "sciences humaines" was an obsession for "la classification yanquie." That is, he considers "sciences humaines" to be a new term introduced as the French equivalent of the supposedly American "social science," a name under which (according to Etiemble) "the Americans assemble history, human geography, normal and pathological psychology, and the different branches of sociology" (but not, it would appear, economics, anthropology, or political science). The editors of Dupré's *Encyclopédie du bon français* (1972) observe that the name "sciences humaines" is perhaps maladroit, since it does not include human anatomy and physiology. "Faute de mieux," they conclude, the new name should be adopted, even though "sciences morales" would "be more logical," although antiquated and even "reactionary."[14]

In Germany , as I have mentioned, the usual distinction is between "Naturwissenschaften" (natural sciences) and "Socialwissenschaften" (social sciences), but in the late nineteenth and early twentieth centuries, there came into general usage an additional distinction, "Naturwissenschaften" and "Geisteswissenschaften," roughly the natural sciences (including mathematics) and the sciences of man or, possibly, the arts and humanities plus the social sciences.[15] Current German usage also includes "Soziologie" and even "Sociologie."[16]

* * *

The use of the term "social science," as opposed to "social sciences," reflects the historical climate of the late eighteenth century and of much of the nineteenth. The emerging subdisciplines which we know as economics or sociology or political science (as opposed to political theory or political history) could then be still considered as part of a general "social science."

In America in the nineteenth century, belief in such a general subject – coupled with the goal of improving society – found expression in a strong Social Science movement which had as its stated aim "to create a special and unified science of human society and human welfare."[17] This Social Science movement has been described as "a non-political attempt to produce a social theory and a methodology which could be used as an intellectual instrument for the betterment of the lot of mankind."[18] Eventually (in 1865) there was formed the American Association for the Promotion of Social Science, on the model of the British Social Science Association and obviously patterning its name on the American Association for the Advancement of Science. In the 1880s specialized sub-disciplines broke away from the parent organization with the formation of the American Historical Society and the American Economic Association, followed by a separate organization of the political scientists. In 1909 the rise of the separate disciplines brought the general association for Social Science to an end.[19]

Another attempt in America to have a single "umbrella" organization for all the social sciences produced the Social Sciences Research Council. The SSRC differed from the older Social Science Association in that it did not set forth an ideal of a unified and general social science, but was created as a cooperative organization of separate and individual social sciences. Traditionally, the social sciences have included five fundamental disciplines: anthropology, economics, political science, psychology, and sociology. When the Social Science Research Council was organized in 1923 as the counterpart of the National Research Council, the core membership consisted of the professional or scholarly associations representing these five disciplines plus two others – history and statistics.[20] History is sometimes classed with the social sciences, sometimes with the humanities.[21] George Homans's list of "social sciences" includes "psychology, anthropology, sociology, economics, political science, history and probably linguistics."[22]

The first article in the *Encyclopaedia of the Social Sciences* (1932), written by the editor, Edwin R.A. Seligman, posits three classes of social sciences – the "purely social sciences" (the earliest ones, in historical order – politics, economics, history, jurisprudence: and the later ones, in historical order – anthropology, penology, sociology, and social work); the "semi-social sciences" (ethics, education, philosophy, psychology); and the "sciences with social implications" (biology, geography, medicine, linguistics, and art). In the Introduction to the successor *International Encyclopedia of the Social Sciences* (1968), the editor, David L. Sills, acknowledges (pp. xxi–xxii) that no final answer can be given to the question, "What are the social sciences?" The reason is that the scope of the social sciences varies from one time period to another. Sills calls attention to certain controversies, e.g., whether history is a social science or part of the humanities, whether psychology is a social or a natural science. The editors, he reports, determined that "the majority of the topical articles" would be devoted to anthropology, economics, geography, history, law, political science, psychiatry, psychology, sociology, and statistics.

Another grouping of disciplines is the "behavioral sciences," a name which came into general use in the 1950s. A major factor in the spread and acceptance of this term was its use by the Ford Foundation in a large-scale and well funded program that was at first unofficially and later officially known as "behavioral sciences." The behavioral sciences, according to Bernard Berelson, is a rubric usually understood to include "sociology; anthropology (minus archeology, technical linguistics, and most of physical anthropology); psychology (minus physiological psychology); and the behavioral aspects of biology, economics, geography, law, psychiatry, and political science."[23]

In *The Behavioral and Social Sciences* (1969), the primary subject areas considered were: anthropology, economics, geography, history, linguistics, political science, psychiatry, psychology, sociology, and aspects of mathematics, statistics, and computation.[24] This may be contrasted with *Knowledge into Action* (1969), where it is said that "historically," five social science have been "central": anthropology, economics, political science, psychology, and sociology. Other disciplines dealing with "social phenomena" are said to be demography, history, human geography, linguistics and social statistics.[25]

In the chapters of our present book, particular social sciences (e.g., economics, sociology) are referred to under their specific names while the terms "social science" or "social sciences" are used either in the nonspecific sense of former times (to include all the "sciences" relating to human behavior and to human societies) or to indicate an all-encompassing "science" that might embrace all human social activities. For the earliest periods under consideration (e.g., the Scientific Revolution in Chapter 4), theories of government or of the state (the works of Hobbes and Harrington) and the conduct of international relations (Grotius) are included under the rubric of "social sciences" because they represent areas of study which later became part of the recognized social sciences.

SOZIALWISSENSCHAFT AND GEISTESWISSENSCHAFTEN

In the twentieth century, the words "Sozialwissenschaft" and "Gesellschaftswissenschaft" can be used for sociology and also for social science. Sometimes "Gesellschaftslehre" or "Soziologie" is used as the direct equivalent of sociology. In the latter nineteenth century, however, there came into general usage a distinction between "Naturwissenschaften" and "Geisteswissenschaften," understood to encompass respectively the natural sciences (including mathematics) and the human sciences (the social sciences and the humanities).[26] Some thinkers and scholars, such as Wilhelm Dilthey in 1883 and Erich Rothacker in 1926, have suggested that "Geisteswissenschaften" owes its invention or at least its diffusion to J. Schiel, who in 1849 used this term for "moral sciences" in his German version of John Stuart Mill's *System of Logic*.[27] In rendering the title of Book VI, "On the Logic of the Moral Sciences," Schiel does write, "Von der Logik der Geisteswissenschaften oder moralischen Wissenschaften," and he generally employs "Geisteswissenschaften" for "moral sciences" in the text.[28] But the appearance of "Geisteswissenschaften" in the translation of Mill's *Logic* in 1849 seems not to have established this usage as definitive since the term is not similarly employed in the later translation of Mill's *Logic* by Theodor Gomperz, who is 1873 rendered the title of Book VI as "Von der Logik der moralischen Wissenschaften" and uses this equivalent in his text.[29] Moreover, Alwin Diemer has shown that "Geisteswissenschaft" was used as early as 1787, that "Geisteswissenschaften" is found in something like its modern acceptation in 1824, and that the modern sense is clearly attested in the distinction made by E.A.E. Calinich in 1847 between the "naturwissenschaftlichen und der geisteswissenschaftlichen Methode."[30]

The Hegelians regarded "Geisteswissenschaft" as "philosophy of spirit" and therefore as a noun in the singular. The term "Geisteswissenschaften" in the plural seems to have come into general usage as part of the development of the idea of "Geisteswissenschaften" as a set of interrelated but independent disciplines. An academic address given by Hermann von Helmholz in 1862 is of particular interest because of the author's eminent contributions to several of the natural sciences combined with his work on philosophy and fine arts. In his address, Helmholz discussed at some length various relations among "Naturwissenschaften" and "Geisteswissenschaften," indicating both their differences and their interconnections.[31] But it is Wilhelm Dilthey who should probably be considered the major figure both in the development of the concept and in the dissemination of the term "Geisteswissenschaften."[32] For Dilthey's term the English rendition until recently tended to be "human studies" but is now increasingly "human sciences."[33] Today "Geisteswissenschaften" may be considered more or less the equivalent of "human sciences" or "sciences of man" (and so somewhat similar to the French "sciences de l'homme" or "sciences humaines"), a rubric that embraces the traditional subjects of philosophy, philology, literary study, jurisprudence, history, and political science, along with the newer subjects of anthropology, archeology, psychology, economics, and sociology. Other fields, such as theology and education, may also be included with prominent subdivisions, such as the study of folklore and the history of art, even being regarded as separate disciplines.

NOTES

[1] There is no universal agreement today on which subjects of knowledge or inquiry should be included among the social or behavioral sciences; see ch. 1, §1 supra.

[2] For the additional problem of "Geisteswissenschaften" see nn. 26–28 infra.

[3] Karl W. Deutsch, Andrei S. Markovits, & John Platt (eds.): *Advances in the Social Sciences, 1900–1980: What, Who, Where, How?* (Lanham [Maryland]/New York/London: University Press of America: Cambridge [Mass.]: Abt Associates, 1986), pp. 149–253.

[4] Charles Francis Adams (ed.): *The Works of John Adams*, vol. 9 (Boston: Little, Brown and Company, 1854), pp. 512, 523, 450.

[5] Keith Michael Baker: *Condorcet: From Natural Philosophy to Social Mathematics* (Chicago: The University of Chicago Press, 1975), Appendix B: "A Note of the Early Uses of the Term 'Social Science.'"

[6] Ibid., p. 391.

[7] Baker (op. cit., ch. 4, esp. pp. 197–202) gives an excellent and succinct presentation of Condorcet's views of the "science sociale." On p. 201 Baker discusses Condorcet's concept of "social science" and indicates how Condorcet contrasted Greek political theory ("a science of facts, an empirical science, as it were") with "a true theory founded on general principles which are drawn from nature and acknowledged by reason." In the course of this elaboration, the term "political sciences" was, not surprisingly, introduced by Condorect along with "social sciences".

[8] Ibid., p. 392. For the reverse situation, in which the German translator of Mill's *Logic* introduced "Geisteswissenschaften" as the German equivalent of "moral sciences"; see the second part of this "Note of 'Social Science.'"

[9] For example, the economist Francis Ysidro Edgeworth wrote in 1881 of economics as one of the "moral sciences"; in the same work he also wrote of social science, using the French term "mécanique sociale," which he hoped would some day "take her place" as the equal of Laplace's "mécanique céleste." See Francis Ysidro Edgeworth: *Mathematical Psychics: an Essay on the Application of Mathematics to the Moral Sciences* (London: C. Kegan Paul & Co., 1881).

[10] Gilbert Chinard: *Jefferson et les idéologues* (Baltimore/Paris: The Johns Hopkins Press; Paris, Les Presses Universitaires de France, 1925), p. 43–44; also Baker (n. 5 supra), pp. 393–394.

[11] J.H. Burns: *Jeremy Bentham and University College* (London: University of London, Athlone Press, 1962), pp. 7–8.

[12] Victor Branford: "On the Origin and Use of the Word Sociology," in *Sociological Papers* (London: Macmillan and Co., 1905), pp. 5–6 quoted in L.L. Bernard & J. Bernard *Origins of American Sociology: The Social Science Movement in the United States* (New York: Thomas Y. Crowell Company, 1943), p. 3.

[13] John Stuart Mill: *A System of Logic, Ratiocinative and Inductive*, 2 vols., ed. J.M. Robson (Toronto: University of Toronto Press; London: Routledge & Kegan Paul, 1974 – Collected Works, vols. 7–8), p. 895.

It should not be thought that Comte composed this hybrid in ignorance, since he was fully aware that he was compounding a mixture of a Greek and a Latin root. But he saw no other way of having the new science define its subject to be society (using the root "*socio-*" from the Latin noun "*socius*") and also declare its stature as a science by a similarity in its final root to such sciences as biology, geology, physiology, mineralogy, and so on.

[14] Fernand Keller & Jean Batany (eds.): *Encyclopédie du bon français dans l'usage contemporain*, vol. 3 (Paris: Editions de Trévise, 1972), p. 2344.

[15] The complex history of the use of "Geisteswissenschaften" is discussed below in the second part of the present Note.

[16] See further L.H. Adolph Geck: "Über das Eindringen des Wortes 'sozial' in die Deutsche Sprache," *Sozial Welt*, 1962, **12**: 305–339.

[17] L.L. Bernard & Jessie Bernard: *Origins of American Sociology: the Social Science Movement in the United States* (New York: Thomas Y. Crowell Company, 1943), p. 3.

[18] Ibid., p. 4.

[19] Ibid., ch. 8.

[20] Ibid., p. 546.

[21] Ibid., p. 658.

[22] George Homans: *The Nature of Social Science* (New York: Harcourt, Brace & World, 1967), p. 3.

[23] Bernard Berelson, "Behavioral Sciences," *International Encyclopedia of the Social Sciences*, vol. 2 (1968), pp. 41–42. See, further, Herbert J. Spiro: "Critique of Behavioralism in Political Science," pp. 314–327 of Klaus von Peyme: *Theory and Politics, Theorie und Politik, Festschrift zum 70. Geburtstag für Carl Joachim Friedrich* (The Hague: Martinus Nijhoff, 1971).

[24] *The Behavioral and Social Sciences: Outlook and Needs* (Washington; National Academy of Sciences, 1969), a report of the Behavioral and Social Sciences Committee (operating under the joint auspices of the National Academy of Sciences and the Social Science Research Council), pp. xi, 19.

[25] *Knowledge into Action: Improving the Nations's Use of the Social Sciences* (Washington: National Science Foundation, 1969), a report of the Special Commission on the Social Sciences of the National Science Board, p. 7.

[26] See Erich Rothaker: *Einleitung in die Geisteswissenschaften* (Tübingen: Verlag won J.C.B. Mohr [Paul Siebeck], 1920; reprinted with detailed foreword (1930); E. Rothaker: *Logik und Systematik der Geisteswissenschaften* (Munich/Berlin: Druck und Verlag von R. Oldenbourg, 1926 – *Handbuch der Philiosophie*, ed. Alfred Baeumler and Manfred Schröter, numbers 6 and 7, collected in part 2, 1927; reprint, Bonn: H. Bouvier & Co. Verlag, 1947), esp. pp. 4–16.

Also Albrecht Timm: *Einführung in die Wissenschaftsgeschichte* (Munich: Wilhelm Fink Verlag, 1973), esp. pp. 37–48 and 137–140; Beat Sitter: *Die Geisteswissenschaften und ihre Bedeutung für unsere Zukunft* ([n.p.]: Schweizerische Volksbank, 1977), esp. pp. 13–17; Wolfgang Laskowski (ed.): *Geisteswissenschaft und Naturwissenschaft: Ihre Bedeutung für den Menschen von Heute* (Berlin: Verlag Walter de Gruyter & Co., 1970); Wolfram Krömer & Osmund Menghin (eds.): *Die Geisteswissenschaften stellen sich vor* (Innsbruck: Kommissionsverlag der Österreichischen Kommissionsbuchhandlung, 1983 – Veröffentlichungen der Universität Innsbruck, 137); Hans-Henrick Krummacher (ed.): *Geisteswissenschaften – wozu?: Beispiele ihrer Gegenstände uud ihrer Fragen* (Stuttgart: Franz Steiner Verlag, 1988); Erich Rothacker: *Einleitung in die Geisteswissenschaften* (Tübingen: Verlag von J.C.B. Mohr [Paul Siebeck], 1920; reprint, with detailed foreword, 1930); E. Rothaker: *Logik und Systematik der Geisteswissenschaften* (Munich/Berlin:

Druck uud Verlag won R. Oldenbourg, 1926 – *Handbuch der Philosophie*, ed. Alfred Baeumler & Manfred Schröter, nos. 6–7, 1927; reprint, Bonn: H. Bouvier & Co. Verlag, 1947), esp. pp. 4–16. See also L.H. Adolph Geck: "Über das Eindringen des Wortes 'sozial' in die Deutsche Sprache," *Soziale Welt*, 1962, **12**: 305–339.

[27] For a more recent historical study, including the usage of Geisteswissenschaften prior to the translation of Mill, see Alwin Diemer: "Die Differenzierung der Wissenschaften in die Natur- und die Geisteswissenschaften und die Begründung der Geisteswissenschaften als Wissenschaft," pp. 174–223 (esp. pp. 181–193) of A. Diemer (ed.): *Beiträge zur Entwicklung der Wissenschaftstheorie im 19. Jahrhundert* (Meisenheim am Glan: Verlag Anton Hain, 1968 – Studien zur Wissenschaftstheorie, vol. 1); A. Diemer: "Geisteswissenschaften," pp. 211–215 of Joachim Ritter (ed.): *Historisches Wörterbuch der Philiosophie*, vol. 3 (Basel/Stuttgart: Schwabe & Co. Verlag, 1974).

On Dilthey, see H.P. Richman: *Wilhelm Dilthey: Pioneer of the Human Studies* (Berkeley/Los Angeles/London: University of California Press, 1979), esp. pp. 58–73; and H.P. Rickman: *Dilthey Today: A Critical Appraisal of the Contemporary Relevance of his Work* (New York/Westport [Conn.]/London: Greenwood Press, 1988 – Contributions in Philosophy, no 35.), esp. pp. 79–82. In the latter (p. 80), Rickman errs in saying that Dilthey "introduced the term *Geisteswissenschaften* as a translation of J.S. Mill's 'moral sciences'"; as I have mentioned, J. Schiel did this in 1849 in his German version of Mill's *System of Logic*.

See also Wilhelm Dilthey: *Einleitung in die Geisteswissenschaften*, vol. 1 (Leipzig: Verlag von Dunker & Humblot, 1883), esp. pp. 5–7: this work is reprinted in Dilthey's *Gesammelte Schriften*, vol. 1 (Leipzig/Berlin: Verlag von B.G. Teubner, 1922; reprint, Stuttgart: B.G. Teubner Verlagsgesellschaft; Göttingen: Vandenhoeck & Ruprecht, 1959, 1962), see esp. pp. 4–6; there are a number of translations including Louis Sauzin (trans.): *Introduction à l'étude des sciences humaines* (Paris: Presses Universitaires de France, 1942), esp. pp. 13–15; Ramon J. Betanzos (trans.): *Introduction to the Human Sciences* (Detroit: Wayne State University Press, 1988), esp. pp. 77–79, also pp. 31–33; Michael Neville (trans.): *Introduction to the Human Sciences*, ed. Rudolf A. Makkreel & Frithjof Rodi (Princeton: Princeton Unviersity Press, 1989 – Selected Works, vol. 1), esp. pp. 56–58. See also Rothaker: *Logik und Systematik* (n. 26 supra), p. 6.

[28] John Stuart Mill: *Die inductive Logik*, trans. J. Schiel (Braunschweig: Verlag von Friedrich Vieweg & Sohn, 1849). This volume is quite rare; I have not been able to consult it directly. A second edition bears an enlarged title, J.S. Mill: *System der deductiven und inductiven Logik*, 2 vols. (Braunschweig: Druck und Verlag von Friedrich Vieweg und Sohn, 1862–1863); see esp. vol. 2, pp. 433, 437–438.

[29] John Stuart Mill: *System der deductiven und inductiven Logik*, trans. Theodor Gomperz, vol. 3 (Leipzig: Fues's Verlag [R. Reisland], 1873 – Gesammelte Werke, vol. 4), esp. pp. 229, 233–234.

[30] A. Diemer: "Die Differenzierung," (n. 27 supra), pp. 183–187, and "Geisteswissenschaften," p. 211.

[31] Hermann von Helmholz: "Über das Verhältnis der Naturwissenschaften zur Gesamtheit der Wissenschaften," *Philosophische Vorträge uud Aufsätze*, ed. Herbert Hörz & Siegfried Wollgast (Berlin: Akademie-Verlag, 1971), pp. 79–108; Hermann von Helmholz: *Das Denken in der Naturwissenschaft* (Darmstadt: Wissenschaftliche Buchgesellschaft, 1968), pp. 1–29; trans. Russell Kahl & H.W. Eve, "The Relation of the Natural Sciences to

Science in General," *Selected Writings of Hermann von Helmholz*, ed. Russell Kahl (Middletown [Conn.]: Wesleyan University Press, 1971), pp. 122–143. On this topic, see David E. Leary: "Telling Likely Stories: The Rhetoric of the New Psychology, 1880–1920," *Journal of the History of the Behavioral Sciences*, 1987, **23**: 315–331.

[32] H.A. Hodges: *The Philosophy of Wilhelm Dilthey* (London: Routledge & Kegan Paul, 1952; reprint, Westport [Conn.]: Greenwood Press, 1974), esp. pp. xxi–xxiii; Michael Ermarth: *Wilhelm Dilthey: The Critique of Historical Reason* (Chicago/London: The University of Chicago Press, 1978), esp. pp. 94–108, 359–360; H.P. Rickman: *Dilthey Today* (n. 27 supra), esp. pp. 79–82; Erich Rothacker (n. 26 supra), esp. pp. 253–277.

[33] Cf. Rudolf A. Makkreel: *Dilthey: Philosopher of the Human Studies* (Princeton: Princeton University Press, 1975), esp. pp. 35–44; H.P. Rickman: *Wilhelm Dilthey: Pioneer* (n. 27 supra), esp. pp. 58–73; and the works cited in nn. 2 and 7 supra.

I. BERNARD COHEN

1. AN ANALYSIS OF INTERACTIONS BETWEEN THE NATURAL SCIENCES AND THE SOCIAL SCIENCES

1.1. INTRODUCTION

Ever since the time of Aristotle, the natural sciences and medicine have furnished analogies for studies of governments, classifications of constitutions, and analyses of society.* One of the fruits of the Scientific Revolution was the vision of a social science – a science of government, of individual behavior, and of society – that would take its place among the triumphant sciences, producing its own Newtons and Harveys. The goal was not only to achieve a science with the same foundations of certain knowledge as physics and biology; there was thought to be a commonality of method that would advance the social sciences in the way that had worked so well in the physical and biological sciences. Any such social science, it was assumed, would be based on experiments and critical observations, would become quantitative, and would eventually take the highest form known to the sciences – expression in a sequence of mathematical equations.

By the end of the eighteenth century, it was obvious that no social science had been created as the equal of Newton's physics, Harvey's physiology, or even the new experimental science of electricity pioneered by Benjamin Franklin. On several occasions, Franklin expressed his awareness of this difference between the social sciences (or "moral" sciences) and the recognized physical and biological sciences. In a letter of 1780 to his friend and scientific colleague Joseph Priestley, he took note of the "rapid Progress true Science now makes" and wished that "moral Science were in as fair a way of Improvement." The century's end brought renewed hope for social or moral sciences that would become equal partners with the sciences of nature. A symbol of this dream may be seen in the establishment of the National Institute in France after the Revolution had dissolved the old Royal Academy of Sciences. The new Institute had several "classes," one of which was equivalent in its membership to the old scientific academy, but another was the new "class" of "moral and political sciences" ("classe des sciences morales et politiques"), as a kind of equal partner. Benjamin Franklin had been

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 1–99.

a "foreign associate" of the old Academy of Sciences since 1773; Thomas Jefferson was elected a "foreign associate" of the new section or class in 1801.

The ultimate fate of this new class of "sciences morales et politiques" is an index of the problems that beset the social sciences. Since social scientists – especially political scientists – cannot help but deal with controversial issues, their opinions and conclusions may become offensive to the ruling powers of the state. Within a very few years of the establishment of the "class," the social and political views of the social scientists in the Institute so ired Napoleon that he reacted by abolishing their class, thus officially severing the ties of social sciences with scientific respectability. The organized physical and biological sciences did not deal with such controversial issues, nor did the group that represented the interests of the members of the old Academy of Inscriptions and "Belles Lettres."

Any historical study of the relations between the social sciences and the physical and biological sciences touches at once on the legitimacy of the several social sciences. A fundamental issue of controversy is whether such legitimacy arises from a slavish adaptation of concepts, principles, theories, and methods from one of the natural sciences (usually considered to be physics) or whether these "other" sciences have their own independent methodologies and standards. In exploring this and allied questions of methodology and legitimacy, our attention will be focused on the late nineteenth century, when two social sciences – economics and sociology – claimed scientific legitimacy because of their use of concepts, principles, and methods of, respectively, physics and biology. An important ground for claiming full membership in the accepted family of "sciences" was a declared general parallelism between these subjects and the accepted sciences of physics and biology, but there was additionally a degree of equivalence of concepts such as energy (utility) or the cell (the social entity of the human individual or the human family). In the case of economics there was even a proud exhibit of equations of identical form with those of physics. We shall see below how these two developments illustrate the two themes of legitimation and of transfer of concept and method.

The present exploration into the impact of the natural sciences on the social sciences leads to several different lines of thought. We shall see that in the late nineteenth and early twentieth century, the physical and biological sciences served two distinct purposes. One was to validate the methodology, the other to guarantee the results. In this enterprise, many founders of the new economics – known today as marginalist or neoclassical economics – chose physics as the science to emulate, but an important school of sociology preferred the biological sciences. The validation of a social science by showing that it is like an accepted natural science may be seen clearly in the example of Léon Walras, one of the late-nineteenth-century founders of the marginalist school of economics. Walras, as we shall see, knew only the most elementary mathematics and very little physics during the decades of the nineteenth century when he was developing his system of economics. It was only later, in the early twentieth century, when he was "hungry" for recognition, that he picked up enough mathematics and physics to claim that his economics was "scientific" and exact because it could produce equations similar in form to those of rational mechanics, the pioneer exact science. Even earlier, William Stanley Jevons had attempted to justify the introduction of the calculus into economics by arguing that this kind of mathematics had been used successfully in rational mechanics – thus implying that economics was like physics because both were susceptible of the same kind of mathematical treatment. Furthermore, Jevons introduced some examples to show that economics, in the form in which he presented it, could be treated like physics, even equating the economics concept of "utility" and the physics concept of "energy."

Validation of a similar kind was sought by those sociologists who adopted the biological sciences as their paradigm. In this emulation, they drew strength from the example of the medical biologist Rudolf Virchow, founder of the doctrine of "cellular pathology," who had introduced social concepts into his medical thought, thus legitimating the association of cell theory and theories of society. Drawing on this association, these sociologists – notably Paul von Lilienfeld, Albert E. Schäffle, Herbert Spencer, and René Worms – constructed a sociology based on such current biological developments as the cell theory, the biological concept of division of labor, medical ideas of normal and pathological, and the physiology of the "milieu intérieure." We shall see that they even introduced little biological tutorials to show the harmony of their ideas with those of the leading biologists of the time.

The marginalist economists differed greatly among themselves with respect to the use of mathematics. The Austrian economist, Karl Menger, for example, did not draw on physics and mathematics. Alfred Marshall, one of the "greats" in this area, preferred a biological to a mathematico-physical model, even though as an undergraduate at Cambridge he had studied mathematics and physics. An important group – including William Stanley Jevons, Léon Walras, Vilfredo Pareto, and Irving Fisher, all of whom claimed that their subject was equivalent to physics – varied greatly in their knowledge of any higher mathematics and the mathematical physics of rational mechanics and energy. Jevons and Walras had, at best, a rudimentary acquaintance with mathematics. Pareto, however, was trained as an engineer and thus, unlike Jevons and Walras, was at home with mathematics and knew some physics. Fisher, who obtained his Ph.D. at Yale, was a student of J. Willard Gibbs and also was qualified as a mathematician. Whereas Pareto and Fisher actually used mathematics in developing their ideas, Walras and Jevons did not, introducing mathematics more as an instrument of legitimation than as a tool of discovery. But the real founder in the application of higher mathematics (i.e., the calculus) to economics was Antoine-Augustin Cournot,[1] who lived somewhat earlier in the nineteenth century and who certainly could not be faulted for his mathematical expertise. We shall have occasion to observe that mathematicians – Henri Poincaré, Henri Laurent, Vito Volterra – criticized the mathematical constructions of the marginalists, challenging the claims that their economics displayed the mathematical integrity of physics.

It is a curious paradox that although the organismic sociologists cannot be censured for their science, their writings seen ridiculous to us today. The marginalist economists are currently under fire for – among other things – not having fully understood the science which they were emulating, yet their ideas are still part of the foundation of today's subject. Furthermore, the kind of physics with which these economists are associated is now outmoded and has been replaced by concepts from relativity and quantum mechanics – subjects that seem not to have permeated deeply (if at all) into today's mainstream economics. Curiously enough, the biological science of the nineteenth century has weathered the years somewhat better than the physics, requiring revisions and expansions but not the same degree of radical restructuring, while the sociology built on the biology has not done as well as the economics which was (in part, at least) linked with the physics. Apparently, the correctness of the emulated science is not intrinsically connected with the permanent value of the resultant social science.

For comparison and evaluation of the different ways in which the natural sciences have influenced the social sciences, at least a rough typology of interactions is required. We shall see that it is sometimes helpful to distinguish between metaphor, on the one hand, and analogy and homology on the other, and also between analogy and homology. The use of metaphor may imply a transfer of values: for example, to demonstrate that economics is a Newtonian science. But analogy implies similarities in function, such as the role of a single great unifying law to explain society in a manner like that of the law of universal gravity in organizing the phenomena of terrestrial and celestial mechanics. Homology, however, implies an identity in form or structure. This similarity, we shall note, may be purely formal. That is, the same equations or principles may appear in two different sciences, which means that there will be an identity of form in which the only differences are the actual letters or symbols in equations or the names of concepts in the statement of principles. In a highly typical example, we shall see that an argument over the economics of "the firm" can be understood by differentiating the use of a general analogy taken from biological evolution and the problems of a specific set of homologies, including such specific concepts as mutation and inheritance. Thus, as we shall note, the distinction between function and form tends to coexist with or modulate into a distinction between the more general and the more specific.

In another example, we shall see why it can be helpful for a critical analyst to make a distinction between analogy and homology in relation to a theory of society. Two major sociologists of the nineteenth century – Paul von Lilienfeld and Albert E. Schäffle – agreed on the importance of using the analogy of the biological cell theory in developing a useful theory of society. They did not, however, reach the same conclusion when it came to the question of specific homology. They parted company on the issue of whether the social homologue of the biological cell was the human individual or the family. In another example that shows why it is important to make a distinction between analogues and homologues, we shall find that Walter Cannon had the laudable idea of applying the results of his research in physiology to social analysis. He wanted to find social analogues of the self-regulating mechanisms which he had been studying in animals and in human beings in his laboratory. So far so good! But he went astray when he sought to introduce specific homologies.

The correctness of the analogues, homologues, and metaphors used by social scientists has never proved to be a guarantee of the validity or usefulness of any social science. Nor is a social science less valid if it does not in any way attempt to imitate a particular natural science, to be like physics or like biology. We shall see, accordingly, that the ultimate criteria for the validity of any social science and the grounds of its usefulness must be independent of the question of whether it is a subject like physics or like biology. Much more important in any evaluation is whether this subject has its own integrity, whether it is internally coherent, whether its results are testable, and whether its assumptions are of the sort demanded by rational explanation. That is, one would not accept a social theory as a science if it depended on the primary postulation of divine intervention. At the same time, of course, a social science which did not take advantage of useful and relevant applications from the natural sciences would be open to severe criticism. Yet this same criticism would be doled out to any branch of the natural sciences that similarly ignored relevant and useful work from other disciplines. Indeed, a development within the social sciences would be equally faulted for ignoring useful and relevant advances in some other of the social sciences. It is the case, however, that a social science like economics – which "looks" somewhat like physics in being quantitative, in finding expression of its principles in mathematical form, and in using the tools of mathematics – tends to rank higher on the scale of both scientists and non-scientists than a social science like sociology or political science which seems less like an "exact science."

1.2. DEFINITIONS AND PROBLEMS

The study of the interrelation between the natural and the social sciences is beset with fundamental difficulties, beginning with the meaning of the two terms: "natural sciences" and "social sciences." Conventionally, the natural sciences comprise the physical and biological sciences, the earth sciences, meteorology, and sometimes mathematics. When I refer without qualification to the natural sciences, I shall be including all of these, from biology and geology to chemistry and physics and mathematics.

The social sciences are generally understood to include anthropology, archeology, economics, history, political science, psychology, and sociology.[2] There is traditionally a third group, the "humanities," embracing such disciplines as philosophy, literary study, linguistic study, and sometimes history. Often the category of science or natural science is extended to some subjects normally regarded as social sciences or as parts of the humanities and may include, in addition to (physical) anthropology and (experimental) psychology, such varied fields as linguistics, archeology, and economics. Sometimes geography is considered a social science, sometimes a natural science. In the last forty years some but not all of the traditional social sciences have come under the umbrella of "behavioral" sciences.[3]

The problem of definition is complicated by the fact that these divisions are not the same in all languages and cultures. Even the designation "science" or "natural science" can give rise to confusion since there are differences in usage among the English "science," the German "Wissenschaft," and the French "science."[4] In English-speaking countries, the term "science" without any qualifying adjective often denotes only the natural sciences considered separate from the social sciences. The Royal Society, the British national "scientific" society, has no membership category for the social sciences[5] and in this respect is even more rigid than its American counterpart, the National Academy of Sciences, which does at present have a recognized category of membership for some social scientists.[6] The French Académie des Sciences is like the Royal Society in excluding social science and is even stricter about admitting non-scientists.[7] In Germany, however, the major academy (the Berlin Academy, founded by Leibniz at the end of the seventeenth century) has always had a broad base of membership.[8] Often in German culture there is a bipartite division of "Wissenschaft" (science or knowledge) into "Naturwissenschaften" (natural sciences) and "Sozialwissenschaften" (social sciences) or into "Naturwissenschaften" and "Geisteswissenschaften" (human sciences).[9]

Furthermore, even within a single language or culture, terms connected with the sciences have not always had the meaning which they bear now. Thus in England as late as the eighteenth and early nineteenth centuries, both "experimental" and "science" were used in a general sense to denote respectively "based on experience" and "system of knowledge,"[10] as well as in senses closer to ours. The older sense of "experimental" and of "science" may be seen in a statement of 1833 by Thomas Babington Macaulay that the "science of government is an experimental science."[11] Macaulay did not mean that this subject was based on laboratory investigations or that it was exactly like physics or biology. For him the science of government was a branch of organized thought, founded on solid experience, especially as revealed by the historical record. A similar use of "experimental" in a political context occurs frequently in the eighteenth century. One example is a letter written by Edmund Burke to the Duke of Bedford, asserting that politics is a "glorious subject" for "experimental philosophy." Another is the subtitle of David Hume's *Treatise of Human Nature* (1739): "an attempt to introduce the experimental method of reasoning into moral subjects." In the introduction to this work, moreover, Hume refers to the "four sciences of *Logic, Morals, Criticism, and Politics*," implying that these subjects are systems of organized knowledge.[12] In Hume's time, the areas of knowledge which we would call science were largely known as "natural philosophy" or "natural knowledge."[13] The term "science" in its present denotation and the associated designation of "scientist" were not introduced into the English language until the nineteenth century and did not become part of general usage until after the 1850s.

For a historian, a striking difference between the natural sciences and the social sciences is the degree to which social scientists still read with profit the classics of their fields, finding an examination of the views of the founders to be instructive and sometimes even necessary for today's subject. So extreme is this practice that James Coleman concludes that university courses in "social theory" today may be regarded as no more than histories of social thought: "An unfriendly critic would say that current practice in social theory consists of chanting old mantras and invoking nineteenth-century theorists."[14] For natural scientists, by contrast, such encounters with the writings of the past are generally held to be unnecessary.

An examination of the literature concerning the relationships between the natural sciences and the social sciences reveals that until fairly recently there was an excessive concentration on whether the social sciences are or are not sciences in the sense of the natural sciences. The experience of many decades has indicated that this is not a fruitful question. Many analysts, such as Hilary Putnam, have insisted that there is no single paradigm which unambiguously applies to all the natural sciences. In most ordinary discourse, the quality of being a "science" is to be like physics. Such an attitude also characterizes the discourse of many scientists – except, of course, naturalists. But even to be like physics has its problems since this category embraces such varied subjects as rational mechanics, experimental optics, and theoretical physics.

There is, in addition, the choice to be made among Newtonian or Einsteinian physics or the physics of quantum mechanics. Probably the one aspect of this question on which most natural scientists would be in agreement is that there is a difference between the natural sciences and the social sciences and that sociology in particular is not a "science" – an opinion held also by certain sociologists.

The question of the social sciences as "sciences" is further complicated by the fact that the answer will depend on the historical period, since the image of what a science is varies from one age to another. Furthermore, one or the other social science may be very like some given natural science and yet be very different from others. Because of the extreme difficulty in setting up hard and fast rules to decide whether a given theory does or does not merit being considered part of "science," we may well understand why, as Robert Merton has indicated, social scientists have allowed this problem "to commit suicide" and have more profitably concentrated on producing "scientific results."[15]

It must be noted, however, that the general problem of definition and delimitation has been of real importance in deciding questions of policy during the past decades. For example, the obvious primary intention of the United States Congress in establishing the National Science Foundation in 1950 was to provide federal support of fundamental research and training in "science," where "science" was intended to signify the traditional natural sciences (including mathematics) and engineering.[16] Many natural scientists at that time were quite vocal in their opposition to the inclusion of any support for the social sciences. For example, the physicist and Nobel laureate, I. I. Rabi, who exerted a very strong influence on questions of science policy, stated bluntly to the Congress, during the debates on the founding of NSF, that government support of the social sciences was inappropriate since it would "strengthen a preconceived point of view or a particular opinion." Additionally, he argued, "most of the things or many of the things which a social scientist has to say are controversial in nature," a feature which – according to Rabi – does not hold for physical science "simply because it is quite objective."[17] Rabi feared that the work done by social scientists, if supported by the new foundation, would reflect adversely on the good work done by natural scientists. Most of the scientific community shared these attitudes.

The hearings with regard to the proposed foundation showed that a significant number of lawmakers opposed support of the social sciences because they tended to equate "social science" with "social reform" and to equate "sociology" with "socialism," a source of confusion that has plagued the social sciences for at least a century.[18] Senator Fulbright, a former university professor, tried to explain to his colleagues that social science is not another word for socialism or "some form of social philosophy."[19] In the event, the attempts to have the social sciences formally incorporated into the National Science Foundation were defeated and the NSF was established by the Congress without any specific provision for the social sciences. A compromise, however, enabled the Director and the National Science Board to exercise full discretion with respect to support of some work in social science within the Foundation, a position that was officially "permissive, not mandatory."

In the inaugural years of the Foundation, the social sciences were all but excluded from aid. Then token support was introduced by an internal administrative decision which permitted direct funding of research in carefully selected areas of the social sciences. First steps in this direction were the extension of the mandate of the biological sciences to include some "behavioral sciences" and the creation in 1955 of a small subdivision of physics (with minimal funding) euphemistically given the neutral designation of "socio-physical sciences." Those of us who were privileged to serve on the inaugural advisory panel of this subdivision represented the history, philosophy, and sociology of science, plus archeology, anthropology, comparative anatomy, political science, sociology and social psychology, and mathematical economics. After some years of steadily increased funds for research, our subjects were incorporated in 1959–1960 into a full-fledged Office of Social Sciences, then reconstituted in 1961 as the Division of Social Sciences, equal in position – though not in prestige, power, or funding – to the other scientific and educational divisions within the Foundation.[20] The National Science Foundation quickly became one of the major sources of funds for research and training in these areas of the social sciences. The existence of this Division, headed by the distinguished sociologist Henry W. Riecken, was a declaration that the social sciences – unlike the humanities – were at last becoming formally and financially recognized as members (although possibly only "associate" members) of the natural sciences establishment.[21]

1.3. TYPES OF INTERACTION

The impact of natural sciences on social sciences involves various components and factors. Among the determinant components are the specification of the area of social science which is to be affected and the choice of the scientific domain which is to provide a source of emulation. These two components are frequently selected together. Another component is the more general one of the scientific climate.

The selection of a particular social science and a particular natural science may be illustrated by a host of examples. In the seventeenth century James Harrington modeled his theory of society on William Harvey's new physiology.[22] Toward the end of the nineteenth century, the economist William Stanley Jevons proposed a new economics based to some degree on the model of Newtonian rational mechanics. In three examples from the last hundred years it was the scientists themselves who designated an area of the social sciences in which their work might be fruitfully applied. The German physical chemist, Wilhelm Ostwald, endeavored to create a new form of social science based on energetics; he called this science "Kulturwissenschaft" instead of the accepted "Sozialwissenschaft."[23] In a somewhat similar fashion the American physiologist Walter Bradford Cannon essayed an extension of his research on self-regulating processes of the human body to social theory, attempting to transform and revitalize the traditional concept of the body politic. In our own time we have seen E. O. Wilson develop sociobiology by generalizing his studies of evolutionary biology and of the group behavior of ants.

A somewhat different example is provided by the British philosopher George Berkeley who – in the eighteenth century – was also working from natural science to social science. He sought to prove that Newtonian rational mechanics might be applied to produce a science of social interactions. This may be likened to the attempt by John Craig, Newton's contemporary, to find a social analogue for law of universal gravity. Adolphe Quetelet, the nineteenth-century pioneer of social statistics, was a professional astronomer who saw in the domain of social numbers a fruitful field for the application of statistical modes of investigation. The opposite path was followed by Emile Durkheim, who discerned in the social numerical data of suicides a statistical base for a science of society.[24]

The more general determinant component, the scientific climate, may be observed in almost every instance in which there is an impact of the natural sciences on the social sciences. In the seventeenth century, the creation of new mathematics – analytic geometry, the calculus, and the use of continued fractions and infinite series – and the outstanding success of a mathematical point of view in physics and astronomy established a mathematical climate, the effects of which are easy to see in the social sciences.[25] Hugo Grotius, whose intellectual ideal was Galileo's new physics of motion, displayed the influence of the mathematical way of thinking in his celebrated treatise on international law. In this climate the French engineer Vauban saw the need for a number-based statecraft. Perhaps the most easily discerned effect of this mathematical climate is the development by Graunt and Petty and their eighteenth-century successors of a numerical approach to the problems of government which Petty named political arithmetic.[26]

In the late eighteenth century, the scientific climate was in some respects even more mathematical. In this era mathematics had two different implications for the natural sciences: to apply actual mathematical procedures in order to derive principles of science from sound axioms and to base science on numbers or on quantitative considerations. Even natural history, that least mathematical subject within the natural sciences, began to incorporate some quantitative features, as we may see in Buffon's celebrated *Histoire naturelle*, where the discussions of anthropology featured the statistical studies of mortality made by Jean-Pierre Emile Dupré de Saint-Maur.[27] The development of a mature science of probabilities, remarkably advanced by Laplace's *Théorie analytique des probabilités* of 1812, was another very significant aspect of the quantitative scientific climate at this time. It had a notable counterpart, of course, in the collection of all sorts of demographic data and social statistics.[28]

The influence of the mathematical climate may be seen also in the concept of an "ideal man," Condorcet's primitive earnest of Quetelet's later concept of "l'homme moyen" or "average man." Condorcet's model for social science, as Keith Baker has shown,[29] embodied the new probabilistic philosophy which made this area of knowledge as susceptible of calculation as the physical sciences, a fundamental step in a sequence that eventually led to Quetelet's statistically based "physique sociale."[30] Baker argues that "the structure of scientific discourse in the eighteenth century not only yielded a probabilistic model of science explicitly applicable to social affairs but, in some ways, required such application as proof of the validity of scientific knowledge."[31]

The intellectual climate, furthermore, includes the standards of knowledge and a system of values that constitute a set of metaphors which determine a style of doing science acceptable to the members of the profession. The introduction of mathematical methods from physics into economics shows how the choice of a metaphor from the natural sciences may condition such acceptability. The "progenitors of neoclassical economic theory" of the latter nineteenth century and early years of the twentieth wished "to preserve the legacy of the classics and to refurbish their thoughts in line with new ideas," and therefore they "boldly copied the reigning physical theories." These words sum up the controversial findings of Philip Mirowski, who has been exploring the interplay of mathematical physics and economic theory in the 1870s and later. These "neoclassicals," Mirowski claims, "did not imitate physics in a desultory or superficial manner," but rather "copied their models mostly term for term and symbol for symbol and said so."[32] The economists Jevons, Pareto, and Fisher declared a goal of making economics a "true" science, choosing physics as a model because this was the science they knew best and because physics was esteemed for its intellectual success and was characterized by the extensive use of mathematics, the primary feature which these economists regarded as making a subject scientific.[33] In this process we not only see the pressure of the intellectual or scientific climate determining a model of mathematics and physics for economics but also discern the value-laden aspects of the particular scientific model chosen by social scientists. The economists apparently did not favor mathematical physics, primarily energy physics and rational mechanics, only because this part of the natural sciences seemed to offer the most fruitful source of useful applications; rather, they opted to emulate a part of the exact sciences that had the highest standing and that could thereby confer upon their own endeavors the quality of legitimacy, showing that their subject exhibited the features of an exact science.[34]

It must not be concluded, however, that even in so physics-like a discipline as economics, the introduction of the techniques and discourse of the exact sciences was an easily acceptable means of showing that one's work was "scientific" or of winning the respect of fellow members of the economics community. Writing about his own experience in introducing the techniques of mathematical thermodynamics into economics, Paul Samuelson has observed that his critics supposed that he was attempting "to inflate the scientific validity of economics," even "perhaps to snow the hoi polloi of economists who naturally can't judge the intricacies of physics." Not so! "Actually," he goes on, "such mathematical excursions, if anything, put a tax on a reputation rather than enhancing it."[35] He had to overcome the impression of being a brash young man and of flouting the agreed-upon rhetoric, metaphors, and standards of technical discourse of his profession.

In addition to the determinant components which we have been considering, the impact of natural sciences on social sciences involves various qualifying factors. These include the degree to which the state of the chosen part of social science permits the desired input from the natural sciences, the degree to which the developments in the natural sciences are susceptible of such application, and the justness of the fit. With regard to whether the chosen part of social science permits the desired input from the natural sciences, an example is once again provided by political arithmetic. Laudable as was the aim of Graunt and Petty to reduce questions of polity to mathematical considerations, the numerical demographic data were not adequate for the purpose and hence did not permit the desired application. By contrast the subject of economics in the mid-nineteenth century proved to be well adapted to the application of mathematical techniques, as may be seen in the successful construction of mathematically based theories by such economists as Edgeworth, Jevons, and Walras.

Whether or not the chosen part of social science is suitable for the application of a particular input often involves the state of development reached by a subject at a given time. One reason why Quetelet had greater success in creating a statistically based social science than Petty or the eighteenth-century political arithmeticians was that in the nineteenth century the actual raw materials of social science – the demographic, census, and social data – were more abundant and reliable than in the eighteenth.[36] Of course, there was the additional factor of the creation of modern statistical methods – in part by social scientist themselves – during the late eighteenth and early nineteenth centuries. Both of these causes for Quetelet's success and Petty's failure are part of the conditioning factor which consists in the state of development of the social science involved.

A second qualifying factor is of the opposite sort from the first one: it is the degree to which the natural sciences have developed to a state that will permit the desired application. The example of political arithmetic exhibits this factor because neither arithmetic nor elementary algebra is sufficient for the analysis of demographic or social data. There was need for a new mathematics, the mathematics of probability, that could be applied to statistical data. The nineteenth-century social scientists who sought to create a numerically based science of society did not wait for a suitable model of applied statistics to emerge from the physical or biological sciences. Rather, since Quetelet and others recognized that the mathematical techniques of statistics had developed sufficiently to permit a wide range of applications, they moved ahead on their own to create a statistically based social science. The high level of statistical social science which they produced then served as a model for emulation in the exact sciences – in the physics of Maxwell and of Boltzmann.

These two qualifying factors partake of opposite facets of the justness of the fit. Of major significance here is the degree of exactness of analogy between some part of social science and some primary concepts from the natural sciences, a topic further explored in the following sections. Or it may be that the structure of some part of the social sciences (for example, economics) may have such a strong formal resemblance to some aspect of the natural sciences (say rational mechanics) that similar equations, laws, and principles may apply to both. This is a familiar situation within the natural sciences; for example, the equations for an alternating current proved to be formally identical to those for an oscillating pendulum. The late nineteenth century witnessed such a fit between a generalized concept of evolution, developed in the context of biological science, and the study of societies or cultures. Many instances of both close and poor fit prove to have two very different aspects, which may be termed analogy and homology.

1.4. ANALOGY AND HOMOLOGY

In considering the interactions of the natural sciences and the social sciences, a useful distinction may be made between analogy and homology and between both of these and metaphor. The word "analogy" is generally used today to indicate many kinds of similarity, but in the natural sciences analogy denotes an equivalence or likeness of functions or of relations or of properties. Thus David Brewster wrote in

1833 about waves or undulations as "a property of sound which has its analogy also in light."[37]

This particular sense of analogy is of special significance in writings on natural history: for example, to express a similarity in function between organs which may seem somewhat different in different species. An example is the wing of a bird as compared with the wing of a bat. Wings of each type enable their possessors to fly, and hence they are analogues; that is, they perform similar functions in both animals, even though a bird's wing is covered with feathers while a bat's wing is a stretched skin membrane.

In the language of the life sciences, the term "homology" has a specific meaning which is quite distinct from that of analogy: to denote similarity in form as distinguished from similarity in function.[38] The distinction becomes apparent once attention is focussed on structure (anatomical construction) rather than function (use in an action).[39] An anatomical comparison of bone-structure shows that the wings of the bat resemble the wings of birds, the forelegs of quadrupeds, and the arms of humans. Hence, the wing of a bird and of a bat, the foreleg of a quadruped, and the arm of a human (and also the pectoral fin of a fish and the flipper of a seal) are homologues. It should be noted that in evolutionary biological science,[40] "homologous" has a strict signification: a correspondence in the type of structure of parts or organs of different organisms resulting from their descent from some common remote ancestor.[41]

In what follows I shall consider the terms analogy and homology as denoting respectively, at their most precise, similarity in function and similarity in form. But the differences between these two kinds of resemblance may result, as will be shown, in a related and sometimes more obvious difference between analogy as suggesting only a general similarity and homology as representing a quite specific one. These distinctions will help to indicate the ways in which the social sciences have used the natural sciences and equally the ways in which the natural sciences have used the social sciences. The same features may be seen in the ways in which the different natural sciences have made use of one another.[42]

Several examples of laws formulated for the social sciences illustrate the distinction between analogues and homologues. A number of social laws in the domains of human behavior, sociology, and economics were proposed as either analogues or homologues of the Newtonian law of universal gravity. The Newtonian law accounted for a number of different kinds of phenomena both in the heavens and on our earth. These phenomena included the orbital motions of planets, planetary satellites, and comets; the occurrence of the tides in the ocean; the fact that, at any given place, bodies of different weights fall at the same rate; the varying of terrestrial weight with latitude; and much else. The Newtonian law states that the force of gravity between any two bodies is directly proportional to the product of the masses of the bodies and inversely proportional to the square of the distance between them.

In the middle of the nineteenth century the French economist Léon Walras and the American economist and sociologist Henry C. Carey proposed laws which can be considered analogues of Newton's to the degree to which both were intended to serve the same basic function in sociology or economics that Newton's law served in rational mechanics and celestial dynamics. Carey's law was presented as a kind of corollary to a general principle of social gravitation: "Man tends of necessity to gravitate towards his fellow-man." His corollary is that "the greater the number [of men] collected in a given space the greater is the attractive force there exerted."[43] Like Newton's law, Carey's expresses a property of an "attractive force." Carey's force is as the number of men in two places, which is formally equivalent to Newton's force as directly proportional to two masses. That is, a force is posited as proportional to a product of two variables; in this sense there is a homology between the two laws. In Carey's law, however, the force is inversely as the distance, whereas in Newton's law the force is inversely as the square of the distance.[44] The two laws, therefore, do not really have the same form; there is not a perfect fit. This kind of failure in homology may be considered an example of mismatched homology, in a sense somewhat analogous to Alfred North Whitehead's concept of the fallacy of misplaced concreteness.[45]

Furthermore, in Carey's law the number of men is an unsatisfactory homologue of Newtonian mass. Mass is the characteristic concept of Newtonian or classical physics and was invented by Newton. Newtonian mass is an invariant property of any body or sample of matter; it does not change when the body is heated or chilled, bent or twisted, stretched or compressed, or transplanted to another location, whether this is another spot on earth or some place out in space or even on the moon or on another planet. In this feature it differs from a local property such as weight, which varies with latitude on earth and also with transplantation to the moon or to another planet.[46] Although Carey's concept fails as a homologue of Newton's mass, it has the same function in his law that Newton's concept has in his law of universal gravity, that is, it shows society functioning in a way that is similar to the way in which Newton shows matter functioning. In short, the two concepts are used analogously even though they are not homologous. But the specificity of comparison and, in particular, the patent attempt to assert a similarity of form between his law and Newton's compel us to characterize Carey's laws as involving an unsuccessful homology.

Let me now turn to Walras's law. Early in his career, in 1860, Walras wrote a short work on "The Application of Mathematics to Political Economy." Here he essayed a Newtonian law of economics, that "the price of things is in inverse ratio to the quantity offered and in direct ratio to the quantity demanded."[47] This law may be considered an analogue of the Newtonian law of gravity in the sense that it is supposed to have the same important role in market theory that the Newtonian law has for the theory of planetary motion; that is, it displays a functional relation between economic entities that has the same functional role as Newton's. But while the two laws may be regarded as analogues in the sense of being functionally equivalent, and even though Walras's law is presented in a form much like Newton's, Walras's law and Newton's are not genuinely homologous. First of all, Walras's law depends on a simple inverse ratio (the price is inversely proportional to the quantity offered), whereas Newton's law invokes the ratio of the inverse square (the force is inversely proportional to the square of the distance). Second, Walras's law involves a direct proportion of a single quantity or parameter (quantity demanded), whereas Newton's law uses the direct proportion of two quantities (the masses). Furthermore, Walras's law posits a price that is proportional to a "quantity" divided by another "quantity" of the same kind or dimensionality, that is, proportional to a dimensionless quotient or pure numerical ratio. Clearly, whatever other characteristics this law may have, it exemplifies a mismatched homology.[48]

The Newtonian social laws of Carey and Walras may be contrasted with Berkeley's attempt to produce a social science based on gravitation. In terms of my earlier discussion of determinant components, I may take note that Berkeley's point of departure was natural science whereas that of Carey and Walras was social science. Moreover, unlike Carey and Walras, Berkeley was an astute student of Newton.[49] Writing in 1713, he began by stating the principles of Newtonian celestial dynamics correctly. This was no mean feat since many social scientists of the eighteenth century, such as Montesquieu,[50] held a totally incorrect view of Newtonian celestial physics. They believed that planets and other orbiting bodies are in a state of equilibrium,[51] a supposed balance between a centripetal and a centrifugal force.[52] Berkeley asserted[53] that society is an analogue (a "parallel case") of the Newtonian material universe and that there is a "principle of attraction" in the "Spirits or Minds of men."[54] This social force of gravitation tends to draw men together into "communities, clubs, families, friendships, and all the various species of society." Furthermore, just as in physical bodies of equal mass "the attraction is strongest between those [bodies] which are placed nearest to each other," so with respect to "the minds of men" – *ceteris paribus* – the "attraction is strongest . . . between those which are most nearly related." He drew from his analogy a number of conclusions about individuals and society, ranging from the love of parents for their children to a concern of one nation for the affairs of another, and of each generation for future ones. Although Berkeley introduced the notion of social attraction and regarded the "minds of men" and the closeness of their relation as having social roles similar to those of mass and distance, he did not attempt to develop an exact homology of concept, nor did he quantify his law of moral force. Perhaps he was thereby spared any possible mismatched homology.[55]

David Hume's *Treatise of Human Nature* (1738) provides an example, similar to Berkeley's, in which there is a general analogue of the Newtonian law of universal gravity without any proposed homology. Hume's goal was to produce a new science of individual human moral behavior that would be equivalent to Newton's natural philosophy.[56] He stated that he had discovered in the psychological principle of "association" a "kind of ATTRACTION, which in the mental world will be found to have as extraordinary effects as in the natural, and to show itself in as many and as various forms."[57] In short, he believed that psychological phenomena exhibit aspects of mutual attraction. But he did not propose a law of mental gravity as a direct counterpart to Newton's law, nor did he propose concepts homologous to those of Newton's *Principia*.[58]

The foregoing examples, in addition to illustrating aspects of analogy and homology, indicate how the natural sciences have influenced the social sciences. In each case there was an attempt to create a Newtonian social science by introducing concepts or laws intended to be analogues or homologues of those used by Newton in his rational mechanics. But whereas Carey and Walras may be characterized as having formulated unsuccessful homologues, Berkeley and Hume may be regarded as having presented only analogues. And there were other social scientists in the eighteenth century and in the nineteenth whose expressed goal was the less specific one: to create a social science that would somehow be the equal of Newton's system only to the extent of organizing the phenomena of society in the same manner in which Newtonian science had organized the phenomena of the physical and cosmic realms.

An outstanding example of such an attempt to produce an analogue of Newtonian science without any homologues occurred in the early nineteenth century in the system of Charles Fourier. Fourier claimed to have discovered an equivalent of the gravitational law, one that applied to human nature and social behavior. Likening his discovery to Newton's, Fourier even alleged that he had been led to his discovery by an apple. He boasted that his own "calculus of attraction" was part of his discovery of "the laws of universal motion missed by Newton."[59]

When, in 1803, Fourier announced his discovery of a "calculus of harmony," he declared that his "mathematical theory" was superior to Newton's, since Newton and other scientists and philosophers had found only "the laws of physical motion," whereas he had discovered "the laws of social motion." Fourier's social physics was based on a system of twelve human passions and a fundamental law of "passional attraction" or "passionate attraction," from which he concluded that only a carefully determined number of individuals could live together in "harmony" in what he called a "phalanx."[60] This Newtonianism was based on a very general Newtonian analogy and contained no homologues of concepts or laws from Newtonian physics.

Emile Durkheim provides another example of a claim to have discovered a social analogue of Newton's law of universal gravity. This emulation of Newtonian physics is all the more surprising in that it appears toward the conclusion of Durkheim's *Division of Labor in Society*, a work exhibiting extensive use of organismic – i.e., biological and medical – analogues of society, even introducing biological cells, physiological functions, the action of a nervous system, and other anatomical and morphological elements. Durkheim's Newtonian social law depends on two social factors: "the number of individuals in relation ["en rapport"] and their material and moral proximity." These factors likewise, are for him, "the volume and density of society"; their increase produces the "intensification which constitutes civilization," or, as he expresses the same idea in a note, "growth in social mass and density" is "the fact which determines the progress of the division of labor and civilization." Durkheim proudly called the sociological law which he had discovered the "law of gravitation in the social world."[61] And one of his formulations of this law certainly is an echo of Newton: "*The division of labor varies in direct ratio with the volume and density of societies, and, if it progresses in a continuous manner in the course of social development, it is because societies become regularly denser and generally more voluminous.*"[62]

Durkheim's law states that "all condensation of the social mass, especially if it is accompanied by an increase in population, necessarily determines advances in the division of labor."[63] That is, in his terms, any increase in social volume or density must result in a heightened competition among similar occupational groups, which will produce a greater division of labor or occupational specialization.[64] Durkheim did not offer evidence of detailed numerical data to support his Newtonian law, nor did he ground it in principles of physics. Rather, he justified the law primarily by means of a biological analogy, a law of Darwin's.[65]

Durkheim's "law of gravitation in the social world" partially resembles Newton's law, since it invokes concepts similar to Newtonian mass, volume, and density. Nevertheless, Durkheim's law does not deal in a Newtonian manner with the interaction of two groups or societies, or with the factor of the distance between the elements of such a pair. He was presumably implying no more than an analogy between the fundamental character of his social law of gravitation and Newton's physical law. He asserted the importance of his discovery of "the principal cause of the progress of the division of labor" by declaring that it has revealed "the essential factor of what is called civilization."[66]

The examples of Durkheim and Fourier, like those of Hume and Berkeley, exhibit a significant feature of the distinction between analogy and homology. Analogies may be useful or useless, appropriate or inappropriate, and moderate or extravagant, and they can be evaluated for their relevance. Homologies, by contrast, are subject to evaluations in terms of correctness rather than relevance, since they imply an identity of form or structure. Carey and Walras proposed laws that were meant to be Newtonian, but that – by objective standards – did not match the original. They were also so specific that they entailed homologies which can be judged whether they were closely matched. Berkeley and Hume were content with rather general analogies and therefore cannot be faulted on grounds applicable to Carey and Walras. And it is the same for Fourier and Durkheim.

Although errors in homology do not occur in Fourier's and Durkheim's sociologies, mismatched homology characterizes another current of nineteenth-century social thought and its twentieth-century overtones, the attempts to produce organismic theories of society. Examples may be found in the writings of such diverse authors as Thomas Carlyle, Johann Caspar Bluntschli, Paul von Lilienfeld, Albert E. Schäffle, René Worms, A. Lawrence Lowell, Theodore Roosevelt, Herbert Spencer, and Walter B. Cannon.[67]

Mismatched homology appears as a prominent feature in Thomas Carlyle's analysis of the problem of society in *Sartor Resartus* (1833–1834). An example is provided by his discussion of the social analogy of the skin:

For if Government is, so to speak, the outward SKIN of the Body Politic, holding the whole together and protecting it; and all your Craft-Guilds, and Associations for Industry, of hand or of head, are the Fleshly Clothes, the muscular and osseous Tissues (lying *under* such SKIN), whereby Society stands and works; – then is Religion the inmost Pericardial and Nervous Tissue, which ministers Life and warm Circulation to the whole. Without which Perocardial Tissue and the Bones and Muscles (of Industry) were inert, or animated only by a Galvanic vitality; the SKIN would become a shrivelled pelt, or fast-rotting raw-hide; and Society itself a dead carcass, – deserving to be buried.[68]

Carlyle appears to have been obsessed with such organismic comparisons drawn from the realms of anatomy and medicine. For him, England was "in sick discontent," writhing "powerless on its fever bed," and the evils of his contemporary world were a kind of "Social Gangrene."[69]

Another nineteenth-century social thinker who was obsessed with extravagant organismic comparisons was Johann Caspar Bluntschli, a Swiss-German jurist who spent a number of years as a professor at Heidelberg.[70] He was author of many books on the state and on society, but his major theoretical work was *The Theory of the State* (1851–1852; 6th ed., 1885–1886), and his most extreme work was his *Psychological Investigations concerning State and Church* (1844).[71] Deeply influenced by the mystic-psychologist Friedrich Rohmer,[72] Bluntschli endowed the state with the sixteen psychological functions that he believed characterized human beings.[73] Convinced that both the state and the church are organisms similar to human beings, Bluntschli quite logically concluded that they both must have all the primary human attributes, including sexual characteristics, the state representing "the male, the church the female element." This attribution of sex led him to a theory of history, based on social-sexual development, in which the historical "evolution" of society and the state followed the pattern of "evolution" of single individuals. Tracing the sexual history of church and state from childhood (the ancient Asiatic empires) through adolescence (the Jews of Biblical times) to early maturity (classical Greece), he found that in Greece[74] "the ecclesiastical organization" matured earlier "than the political institution," just as "the girl ripens earlier than the boy." So extreme is Bluntschli's mismatched homology that a reader may find it difficult to imagine that he was developing a social parallel when he went on: "The sexual organs of the girl are sooner developed than those of the boy. The youthful breasts begin to swell; and the unfolding virgin turns into a beauty. Beauty was the soul of the cult of the Hellenes. . . ."[75] Bluntschli's attitude towards the sexes led him to assert that the papal desire to subordinate the state to the church is as "unnatural" as "the subordination of a husband to his wife in a household." He envisaged a time, not far off, when the "male state will reach full selfhood," when the "two great powers of humanity, state and church, will appreciate and love each other, and the august marriage of the two will take place."[76]

A similar extravagance occurs in the organismic conception of society proposed by the Russian sociologist, Paul von Lilienfeld, in the comparison which he made between the intellectual and moral state of a hysterical woman and a condition of society.[77] As the physiological foundation of this likeness, he used in particular the findings reported by Dr. Edmond Dupouy (ca. 1845–1920), author of numerous works on medicine, psychology, and medical history. Quoting Dr. Dupouy, Lilienfeld described the condition of women suffering from hysteria.[78] They are, he noted, "mobile in their sentiments," and "they pass very easily from tears to laughter, from excessive joy to sadness, from passionate tenderness to haughty rage, from chastity to wanton purposes and lewd ideas." Additionally such women "love publicity, and to get themselves talked about they employ every means: denunciation, simulation of infirmities or sicknesses, and the revolver." They find joy in pretending to be "victims of anything; they say they have been violated." In order to "achieve their goals they deceive everyone: husband, family, confessor, examining magistrate, and their doctor."[79]

The reader who is uninitiated in the literature of organismic sociology may wonder what social manifestations could possibly be the counterparts of these symptoms. Lilienfeld develops the comparison by presenting a series of correspondences which clearly must be characterized as homologues even if he refers to analogy. He begins by asking rhetorically whether the symptomatic behavior of women suffering from hysteria is not "perfectly analogous to the manner in which the population of a large city behaves during a financial crisis or on the occasion of civil disturbances." He finds in the behavior of such women "a faithful picture of the agitation of parties during elections." And when we consider the past, he asks, do we not find the same confused and disordered pattern of behavior, "caused by convulsive and contradictory reflexes of the social nervous system," during "all the religious, economic, and political revolutions with which humanity has been assailed?"[80] This complex nesting of mismatched homologies needs no comment.

Two authors of very different sorts, one from the nineteenth and one from the twentieth century, provide additional case histories that illustrate the easy susceptibility of social thought to mismatched homology. The first, Herbert Spencer, was a self-educated sociologist and philosopher; the second, Walter Bradford Cannon, was an eminent scientist who dabbled in sociology.

Herbert Spencer[81] indulged himself in analogies and homologies. An extreme example of mismatched homology, which even his sympathetic biographer must admit is a case of "dubious biology . . . added to pedestrian sociology," is Spencer's likening of "the coalescence of the Anglo-Saxon kingdoms into England" and the formation of crustaceans.[82] Here he was introducing his own odd notion that crustaceans, like insects, are "composite animals," in which the segments are independent life-units joined together.[83]

Although he also drew on parallels from the physical sciences, organic correlations permeate Herbert Spencer's writings on sociology.[84] Two samples of his extremes in the production of homologues are (1) his comparison of "the undifferentiated and fragmented structures of Bushmen" with "the protozoa" and (2) his likening of "the ruling class, the trading or distributive classes, and the masses" to "the mucous, vascular and serous systems of the liver-fluke."[85] Perhaps the limit is reached when he refers to the two great national schools of France as "a double gland" intended "to secrete engineering faculty for public use."[86] This final Spencerian example is comparable to one introduced by René Worms in the early twentieth century, and based on regeneration in marine animals such as starfish. Citing the authority of Spencer, Worms compared the way in which certain animals replace a destroyed or damaged organ with Chancellor Maupou's dismissal of the Parlement of Paris and its replacement by a new assembly.[87]

The example of Walter Cannon is more interesting than that of Herbert Spencer because Cannon was one of the foremost scientific investigators of his time. His first essay in biological sociology (1932) was titled "Relations of Biological and Social Homeostasis,"[88] an exploration of whether equivalents of the "stabilizing processes" in animal organisms can be found in "other forms of organization – industrial, domestic or social." In a manner reminiscent of Spencer and other nineteenth-century organicists, Cannon compared the circumstances of small groups of humans living in "primitive conditions" to the "life of isolated single cells," and the grouping of "human beings . . . in large aggregations" to cells "grouped to form organisms."[89] Only in highly developed organisms, he reported, do the "automatic processes of stabilization" work "promptly and effectively." The comparison seemed to show that our present social system resembles organisms low on the evolutionary scale or organisms that have not fully developed, in both of which "the physiological devices which preserve homeostasis are at first not fully developed."

Cannon's major field of scientific investigation was the study of self-regulating processes in the human (and animal) body, stressing the role of the "milieu intérieure." Accordingly, his announced goal in studying social systems was to find in "a state or nation" an "equivalent" for the "fluid matrix of animal organisms." And it is here, in the suggestion of an analogy, that Cannon reveals the naive quality of his social thought. In the social body, he wrote, the equivalent ("in a functional sense") of the fluid matrix for maintaining homeostasis in the living body, is

> the system of distribution in all its aspects – canals, rivers, roads and railroads, with boats, trucks and trains, serving, like the blood and lymph, as common carriers [on which] the products of farm and factory, of mine and forest, are borne to and fro.[90]

Although Cannon sought to limit his comparisons to functional analogies, he unwittingly fell into the trap of mismatched homology by making his analogies far too substantive. He simply could not restrain himself from introducing homologies when he was comparing the cells in an organism with the members of a social group, or the lymph and blood with the system of canals, rivers, roads, and railroads.

Cannon's essay illustrates the danger of using apparent likenesses. On the level of general analogy, his suggestion that society resembles an organism could be regarded as original and instructive, at least by implying that the stability of a society is caused by certain self-regulating mechanisms. We may agree with Robert Merton, however, that Cannon made the mistake of introducing "substantive analogies and homologies between biological organisms and social systems." Merton went so far as to describe Cannon's result an "unexcelled . . . example of the fruitless extremes to which even a distinguished mind is driven." This comment is all the more significant in that it occurs in Merton's essay on "Manifest and Latent Functions,"[91] in which he finds "Cannon's logic of procedure in physiology" to be a model for the sociological investigator, recommending that his readers study Cannon's book on the *Wisdom of the Body*, while warning them about "the unhappy epilogue on social homeostasis."

Almost ten years later Cannon returned to this topic, choosing it as the subject of his presidential address to the American Association for the Advancement of Science, delivered in December 1941.[92] In preparing the new version, Cannon sought help and advice from a sociologist, his junior colleague Robert K. Merton, who sent him a list of books and articles on the subject of society as an organism. Cannon now withdrew his earlier assertions about similarities between cells and human members of society, and he declared that comparisons of "the body physiologic and the body politic" had been discredited in the past because they had mistakenly concentrated on "minutiae of structure."[93] He came out strongly against what he considered to be absurd (we would say "mismatched") homologies. We are "not illuminated," he said, "by a likening of manual laborers to muscle cells, manufacturers to gland cells, bankers to fat cells, and policemen to white corpuscles." He, accordingly, would not be concerned with structures but would rather examine "functional accomplishments in physiological and social realms." Yet, when he posed once again the earlier question of what "corresponds in a nation to the internal environment of the body," his reply was essentially the same as before: "The closest analogue appears to be the whole intricate system of production and distribution of merchandise."

In his presentation of the nation's equivalent of the body's fluid matrix, Cannon now omitted canals and boats (although he kept the rivers) and added "all the factors, human and mechanical, which produce and distribute goods in the vast and ramifying circulatory system which serves for economic exchange." In less florid prose than before, he said: "Into this moving stream, products of farms and factories, of mines and forests, are placed at their sources, for carriage to other localities." His own display of substantive analogies or mismatched homologies was as unfortunate as it had been in the earlier presentation. As the lawyers say, *Res ipsa loquitur.*

In considering these examples of mismatched homology, our evaluations may be sharpened by attention to the reasons why they seem outré to a critical reader. Why do we smile and assume a condescending air when we read the writings of organicist sociologists like Bluntschli, Lilienfeld, and Spencer, but not when we encounter physical models such as Jevons's lever or Walras's economic machine, both of which will be discussed below, or the numerous attempts to find in the realms of social sciences an analogue of the Newtonian universe? The reason is not simply that one set is biologically based while the other set comes from physics. Henry Carey's attempt to produce a sociology based on electricity, a later rival to his astro-sociology, may provoke our smiles and giggles just as easily as the systems of the organicists.[94]

I believe that our pejorative evaluation of certain social comparisons is based at least partly on the fact that the biological equivalent is usually a real object, an actual living being, endowed with all the forces of life and subject to all of life's problems, such as disease, aging, anxieties. By contrast, the parallels from physics are not concrete but abstract and theoretical. Jevons's lever is actually a mathematical lever and thus does not have such material properties as color, hardness, weight, or physical dimensions other than length. Correlations based on a gravitational universe make use of abstract concepts, just as Newton did in Book One of the *Principia.*[95] That is, in Book One there are no real planets with material sizes, shapes, and similar properties but only mass points whose properties are position in a mathematical space, mass, and the power to give rise to, and to be acted on by, a gravitating force. Thus, unlike the earthy biological sources of comparison, those from physics tend to be abstract[96] and may even serve primarily as sources of equations.[97]

Where Bluntschli, Lilienfeld, and Spencer argue that society is itself an organism or is very much like an organism, the "mechanical economists" – Stanley Jevons, Léon Walras, Vilfredo Pareto, Irving Fisher – declare that economics is analogous with mechanics because of the close similarities between the equations of economics and those that originate in classical mechanics. The problem with the organicists' conception of society is, therefore, not that they found their parallels in living systems but that they did not place their considerations on a plane of abstraction, as did those who drew on analogies from physics. They were extravagant because their goal was to create a homology rather than a general analogy. Their procedure is very much like what Whitehead described as "the accidental error of mistaking the abstract for the concrete," mistaking the abstractions of social theory for the concreteness of an actual biological organism. It is not an error to make use of organicist analogies (or in figures which we shall study below as metaphors) in discussing society at large, the political system, or the economic system. People constantly use expressions deriving from the organic notion of the body politic, such as head of state, nerves of government, healthy state of society or of the economy, consumption, arteries, and many others. Werner Stark, one of the severest critics of organicist theories of society, who describes Lilienfeld's theories as "ravings" and "nonsense," nevertheless admits that in writing about certain aspects of society

> one is constantly tempted to express them in organismic similes: phrases like 'one sector limps behind' or 'one sector is out of joint with the rest' tend to form themselves, as of their own volition, in one's mind, and try to push themselves into, and to flow out of, one's pen. This alone shows that organicism has a deep root, and that its basic metaphor is not absurd, even if its votaries make it so.[98]

Much of today's discourse on society, social problems, or systems of social thought, and on political systems and the state, continues to make use of images related to living systems even though usually there are no longer any of the extremes that characterized the nineteenth-century organicists and some of their early twentieth-century successors. Current usage tends to be on the level of analogy and metaphor and not of homology, making use of the general and the abstract rather than the specific and the concrete.[99]

1.5. METAPHOR

Thus far we have been considering analogies and homologies, but we have not yet addressed the general problem of metaphor.[100] When we treat metaphor in relation to the interactions between the natural and the social sciences, it is sometimes useful to make a distinction among four levels of discourse involving comparison. One extreme level is metaphor, the other is identity, with analogy and homology as intermediary. These four levels of discourse may be easily illustrated by reference to biology and physics as utilized in the social sciences.

First, identity. "What is a society?" asked Herbert Spencer; his reply was, "An organism."[101] Two others of the "identity" persuasion were Otto Bluntschli, who, as we have seen, endowed society and its institutions with sex, and Paul von Lilienfeld, who, as we shall see, declared explicitly – for example, in the title of one of his major works – that he considered society to be a "real" organism. Also to be placed in this category are Albert Schäffle, despite some qualifications which he made in theory, and René Worms, at least in his earlier phase. Those whose belief was at the other extreme merely wrote figuratively of society as generally like an organism or as like an organism in some specific respects; they adopted an organismic metaphor. Their number includes Emile Durkheim, Walter B. Cannon, and René Worms in his later works. The level of metaphor has been a consistent feature of the concept of the body politic, which has successively illustrated the changes in physiology and medicine, being Galenic until the seventeenth century, then Harveyan, and so on.[102]

Traditionally, a metaphor is a literary figure of speech, aesthetic or rhetorical. For Aristotle, a metaphor gives something a name that properly belongs to something else.[103] Because metaphor and analogy both invoke features of similarity as well as contrast, it is easy to understand why a clear distinction is not always made between them. Historically, these two were closely linked; Aristotle held that an analogy is only a special case of metaphor.[104] Furthermore, even a specificity akin to that of homology may be regarded as metaphor if the usage is primarily literary – that is, aesthetic or rhetorical – rather than being chiefly an aspect of logical argument.

Metaphor has long been used as a rhetorical device to enhance oral and written communication so as to increase the effectiveness of the message delivered, but during the Scientific Revolution of the seventeenth century rhetoric fell into disfavor. The advocates and practitioners of the "new philosophy" held that science should be presented in unadorned descriptive terms of experiment and observation, followed by strict inductions or deductions, in which each step was to be plain and clearly understood – without any rhetorical flourishes to distract the reader from the evidence and the logic. This was one of the reasons for the great esteem given to mathematics, which is perhaps the most rhetoric-free discourse imaginable.[105]

A classic example of metaphor – the assignment of a descriptive term to some object to which it is not strictly applicable – is the Scriptural comparison of life to a pilgrimage. Perhaps the most famous such metaphor is Shakespeare's comparison of life to a stage. Common metaphors include heart of flint, sharp mind, head of state, leaving no stone unturned, and eye of the law. A striking metaphor was used by James I soon after gaining the crown of England. "I am the husband," he told Parliament, "and the whole Isle is my lawful wife; I am the head and it is my body."[106]

The use of a metaphor does not necessarily imply any technical or scientific knowledge. When we use the metaphor of a marble brow, we mean only that we consider the flesh-and-blood brow to be cold and white like the brow of a marble statue; in this context we need not know anything about the chemistry or structural qualities of marble or the nature of the epidermis. But since a metaphor may also be based on erudition, a helpful distinction can be made between a popular or rough or untechnical metaphor and one that more learnedly invokes some element of the natural sciences. The difference between the two may be seen clearly by considering "body" in the metaphor of the body politic.[107] An example of a non-technical metaphor, one that does not involve the natural sciences, is found in 2 Corinthians, where St. Paul set forth a hierarchy of organs and parts of the body – from head and heart to limbs and belly – without any reference to medicine or physiology. It is the same for the oft-repeated Aesopian fable of the feet and the belly, in which the feet revolt because they believe they do all the work while the belly merely lies at ease above them doing nothing useful.[108] These examples may be contrasted with a statement in which James I likened the expanding metropolis of London to the spleen, "whose increase wastes the body." For here he was basing his metaphor on a physician's acquaintance with the function of the spleen. That is, he was invoking a resemblence between the operations of a city and the functions, considered technically, of an organ of the human body.[109]

All four levels of discourse may be discerned with respect to social applications of Newtonian physics.[110] First, there is the possibility of identity, a belief that the social world is a mechanical system operating under the same principles as the Newtonian system of the world.[111] Additionally, there were attempts, such as those we have seen made by Carey and Walras, to produce Newtonian homologues, laws in the social realm having the form of Newton's law of universal gravity; by contrast, Hume, Fourier, and Durkheim held only that they had produced a law which would have a function in a science of society that was an analogue of the function of the law of gravity in the Newtonian system. Others, however, merely believed that, on the level of metaphor, sociology or economics should be a "science" that, in some unspecified manner, would organize the subject in the way that Newton's *Principia* had done for the physical sciences. This was, apparently, the intent of Hamilton's *cri de coeur* of 1866:

> Although far more advanced, relatively, in particular ideas than sidereal philosophy before the time of Newton, it [social philosophy] scarcely less needs the PRINCIPIA MATHEMATICA PHILOSOPHIAE SOCIALIS, or rather the PRINCIPIA PRIMA.[112]

In explanation, Hamilton set forth what he believed to be the "Newtonian idea of Sociology," the assertion of

> the universality of the causes, or laws, which determine the social condition of mankind, and the consequent identity of the causes which determine the social destiny of an individual and a nation.

A variety of the Newtonian metaphor that has proved to be of significance for the social sciences consists of a Newtonian paradigm for social science based on Newton's method in general, using a procedure which I have called the Newtonian style.[113] This "style" does not refer to the set of mathematical techniques used by Newton – geometry and trigonometry, algebra, proportions, infinite series, and fluxions – but rather to the stages of contrapuntal interactions between imagined or ideal systems and those observed in physical nature.

The *Principia* begins with an idealized world, a mental construct comprising a single mathematical particle and a centrally directed force in a mathematical space. Under these idealized conditions, Newton can freely develop the mathematical consequences of the laws of motion which are the axioms of the *Principia*. At a later stage, after contrasting this ideal world with the world of physics, he will add further conditions to his intellectual construct: for example, by introducing a second body which will interact with the first one and then exploring additional mathematical consequences. Later, he will once again compare the mathematical realm to the physical world and revise the construct, for example, by introducing a third interacting body. In this way he can approach, by stages, nearer and nearer to the conditions of the world of experiment and observation, introducing bodies of different shapes and composition and finally considering bodies that move in various types of resistant mediums rather than in free space.

The *Principia* thus displays both the physics of an ideal world and the problems that arise because ideal conditions differ from the world of experience. For example, Newton shows that Kepler's first two laws of planetary motion are exactly true only for the mathematical or ideal condition of a single mass-point moving about a mathematical center of force, and he then develops the actual ways in which the pure form of Kepler's laws must be modified to fit the world of observation. The *Principia* can be accurately described as a work in which Newton explores, one by one, the ways in which ideal laws must be modified in the external world of experiment and observation.

A somewhat similar procedure was adopted in Thomas Malthus's *Essay on Population.*[114] Malthus stated a basic principle that "Population, when unchecked, increases in a geometric ratio." A later version says that "all animals, according to the known laws by which they are produced, must have the capacity of increasing in a geometrical progression."[115] This law is plainly not the result of a Baconian induction from a mass of observations. In fact, the law is true only of an unchecked population; a good part of Malthus's *Essay* is in fact devoted to evidence that populations do NOT so increase and to explanations of why they do not.

Malthus does not say that observed populations actually increase in a geometric or exponential ratio; he says explicitly that this *would* be the case for populations whose growth was not checked. The similarity of this statement and Newton's first axiom or law of motion will be immediately apparent. Newton did not write that all bodies move uniformly straight forward or stay at rest. Rather, he said that a body will maintain one or the other of those two "states" except to the extent to which impressed forces cause a change in state. Malthus is following the style of the *Principia* in seeking the reasons why the laws of the world of nature differ from those of the world of pure abstraction, in studying why real populations do not increase geometrically as they would in an ideal or imagined world.

In the *Essay* Malthus linked his presentation of the laws of population growth with Newton by citing Newton in terms of the highest respect even though Newton never wrote a word about populations or their increase. Malthus, we may note, excelled in mathematics and mathematical physics while an undergraduate at Cambridge, where he studied the *Principia* as well as commentaries on the Newtonian natural philosophy.[116] His use of Newton shows that the Newtonian natural philosophy has exerted a fruitful influence on the social sciences: not as a source of analogies or homologies, but in that metaphorical fashion which I have called the Newtonian style.

Those who have been engaging in critical-historical analysis of the social sciences, particularly economics, have not always made a clear distinction between analogy and homology, although the cases on which they have focused their critical attention – aspects of marginalist or neoclassical economics – exhibit examples of both. In particular, these analysts stress metaphor. In their usage, metaphor embraces both analogy and homology, but it may go even further to comprise the whole gamut of concepts, laws, theories, techniques, models, standards, and even values of the natural sciences (including mathematics) that economists have sought to borrow, emulate, imitate, or use in any way.

A close examination of the late nineteenth-century marginalist or neoclassical economists shows clearly the dual role of general metaphor and of specific analogy or homology. In the nineteenth century Isaac Newton still symbolized the highest level of scientific achievement, and the words used in relation to Newtonian science – "rational," "exact," and even "mathematical" – denoted a science at the zenith of the scientific hierarchy. Thus the emulation of Newtonian "rational mechanics" (complemented by the addition of such principles as those of Lagrange, d'Alembert, and Hamilton, together with such non-Newtonian concepts as energy) was an act of linking economics with the most successful branch of the natural sciences. This association was based on a metaphor. At the same time, however, rational mechanics provided concepts, principles, and even equations for which there seemed to be useful counterparts – both analogues and homologues – in economics.

Such explorations bring to our attention a very important aspect of interactions between the natural sciences and the social sciences, the transfer of value systems. William Stanley Jevons defended his attempts to introduce mathematics into economics by declaring that differential equations had been used traditionally in rational mechanics. In this statement Jevons was accomplishing two separate aims. He was justifying the introduction of the calculus into a social science and he was also implying that economics is like rational mechanics, then considered the paradigmatic exact science which all others should try to emulate. In short, he was implying that his subject shared the values of that branch of science which was then considered to represent the pinnacle of exactness and success.[117]

The value-laden aspect of metaphor often shows itself clearly in shifts of the specific part of the natural sciences which the social sciences are supposed to emulate. In this connection, the case of Jeremy Bentham is very illuminating. At different times in his life, he considered that the art-and-science of society should be modeled on medicine, even writing that "the art of legislation is but the art of healing practiced on a large scale," which – he added – was not a "mere fanciful" image. But at other times he chose as his paradigm the new chemistry, even conceiving that he would be its Lavoisier.[118] In one case he was lauding the beneficial practise of curing disease and maintaining health; in the other, the radical restructuring of knowledge.

We may see this feature of metaphor even more clearly in Engels's two eulogies of Karl Marx. At Marx's graveside, Engels's laudation took the form of a comparison with Darwin, indicating both the great effect of Darwin and Marx on contemporary thought and the revolutionary character of their ideas. Later, when he was editing Marx's *Nachlass* to produce the second volume of *Das Kapital*, Engels changed his metaphor for Marx's place in history. Now, as he wrote in his introduction, he found that Marx's counterpart was Lavoisier, the chief author (as Engels spent several paragraphs proving) of the Chemical Revolution.[119] While both Darwin and Lavoisier were symbols of scientific greatness, they represented quite different kinds of science and evoked metaphors comprising dissimilar sets of values and achievements. Both Darwin and Lavoisier were responsible for revolutions, but these were of very different types. Darwin radically altered our concept of species and their permanence, and his ideas challenged the existing order of thought in many fields of knowledge and belief. Lavoisier re-ordered the science of matter, and his work caused us to have a new and very different perspective on the constitution of substances, requiring that all substances, natural or synthetic, be given new names. Darwin turned an existing science upside down, but Lavoisier created a new science. Lavoisier made a legitimate science out of an old subject, just as, according to Engels, Marx had done in creating "scientific" economics.

Metaphors imply many aspects of the ways of doing science, the factors that must be considered whenever the historical or analytical focus is broad enough to encompass the total social and intellectual matrix in which science – whether natural or social – is done. Such considerations belong to the general historical interpretation of the sciences known commonly today as "external."[120] It has been argued that a primary reason why the "energy metaphor" was adopted by the neoclassical economists was not that it provided an accurate equivalent but that it invoked the values associated with the system of physics.[121] We are thus reminded that the choice of a particular metaphor to describe the interactions of the natural and the social sciences may suggest systems of values that are just as important as, or that may even be more important than, the compatibility of the concepts, principles, and quantitative elements.

1.6. ROLES OF ANALOGY

Analogies and similar types of correlation constitute a primary means of interaction between the natural sciences and the social sciences. These interactions are very much like those that occur between one branch of the natural sciences and another. They arise from a recognition that an idea, concept, law, theory, system of equations, method of investigation, mathematical tool, or any other element of one subject is similar to some element in another or has properties that enable it to be introduced usefully into that other subject. Analogy has always functioned as a tool of discovery, reducing a problem to another that has already been solved or introducing some element or elements that have proved their worth in a quite different area of knowledge. Jeremy Bentham once said that hints from analogies constitute one of the most important tools available for scientific discovery.[122]

A traditional use of analogy is to justify a novel or radical method or theory. An example would be the introduction of higher mathematics (e.g., the calculus) into economics on the analogy that the calculus had been used successfully in rational mechanics. A related use of analogy is to help explain abstruse concepts, as may be seen in all general presentations of relativity theory. Analogies also serve to make a difficult or strange idea seem reasonable and hence acceptable to the scientific community. An instance occurs in the work of Sigmund Freud.

Freud was hesitant about presenting in full one of his radical and difficult concepts, introduced only as a "suspicion" in 1890 in his *Interpretation of Dreams*. This was the idea that human beings have two different memory systems, one of which, as he wrote in 1924, "receives perceptions but retains no permanent trace of them," while the other preserves "permanent traces of the excitations" in "'mnenomic systems' lying behind the perceptual system."[123] By 1924 he had discovered a mechanical device called the "Mystic Writing-Pad" (an older version of what in the United States is still called a magic slate) which seemed to simulate some main features of his concept. Emboldened by this encounter, Freud described his ideas about human memory in full, suggesting that the writing pad could be considered an analogue of his "hypothetical structure of our perceptual apparatus."[124]

Analogies were of significant importance in Freud's thinking and exposition. The "standard" edition of his collected works, in fact, contains a separate index for analogies. Best known of Freud's analogues are those which he drew from literature, notably Greek tragedy, in formulating and describing (and even naming) concepts. Freud was aware that in his cultural and anthropological studies – e.g., *Totem and Taboo* and *Moses and Monotheism* – "we are only dealing with analogies," and he fully recognized how dangerous it is, "not only with men but also with concepts, to tear them from the sphere in which they have originated and been evolved." It has been observed that by invoking an analogy Freud "likened religion to a collective obsessional neurosis, or allowed that Hamlet suffered unduly from an Oedipus complex."[125]

The explicit use of analogies was introduced into science during the formative years of the Scientific Revolution. In an extensive study of this subject, Brian Vickers has found that in the late Renaissance and early seventeenth century, the attitude toward analogies constituted a major issue on which the new science diverged from an occult tradition.[126] The new science, according to Vickers, stressed a "distinction between words and things and between literal and metaphorical language." In the occult tradition, however, words were "treated as if they are equivalent to things and can be substituted for them." As a result, analogies were not, as they were "in the scientific tradition, explanatory devices subordinate to argument and proof, or heuristic tools to make models that can be tested, corrected, and abandoned if necessary"; they were, instead, "modes of conceiving relationships in the universe that reify, rigidify, and ultimately come to dominate thought." I would modify this conclusion only to the degree of adding that for scientists analogy also served as an instrument of discovery.

One of the early scientists to make extensive use of analogies was Johannes Kepler, who wrote, in his epoch-making work on optics, "I especially love analogies, my most faithful master."[127] In the same work, Kepler indicated how analogy is used in the process of discovery: "Analogy has shown, and Geometry confirms." He employed analogy especially in his *Astronomia Nova* in 1609, where he set forth his first two laws of planetary motion. Reasoning, as he said, "by analogy," Kepler made use of the properties of such "intangibles" as light and magnetic force in order to develop the idea of a solar (or "solipetal") force acting on the planets. He was clear about the distinction between analogy and identity, even stating, with respect to his postulated magnetism of planets: "Every planetary body must be regarded as being magnetic, or *quasi*-magnetic; in fact, I suggest a similarity and not an identity."[128]

Newton also reasoned in terms of analogy and even formalized the use of analogies in natural science in his *Principia*, in the second of what he called the "Regulae Philosophandi" or "Rules of Natural Philosophy." The "causes to be assigned to natural effects of the same kind should" he wrote, "be so far as possible, the same." The examples he gave were "respiration in man and beast," "the falling of stones in Europe and America," "the light of a kitchen fire and of the sun," and the "reflection of light on our earth and in the planets."[129]

A comparable way in which analogies serve science is in exhibiting the validity of a conclusion that seems untestable. In discussing the stability of the solar system in his *Système du monde*, Laplace had to argue that certain observed variations are not secular but periodic; they seem to be secular only because they have a period extending over millions of years. Laplace showed that the system of Jupiter's satellites in a dynamical analogue of the solar system, the satellites displaying in their motions the same perturbations as the planets. Since the satellites exhibit all the phases of their mutual gravitational perturbations within a few centuries, the periodic nature of the oscillations can be verified, thus making it likely by analogy that the similar variations in the planetary motions are also periodic.[130]

Both Charles Darwin and his contemporary James Clerk Maxwell made frequent use of analogies. Darwin's basic concept of a "struggle for existence" was presented in the *Origin of Species* (1859) on the basis of analogy with Malthus's principles of population. Malthus's two laws dealt only with human populations, which if unchecked would naturally increase in an exponential ratio. By analogy Darwin inferred that all populations of organic beings – human, animal, plant – would naturally increase exponentially so that, as he wrote, "more individuals are produced than can possibly survive," with the result that "there must be a struggle for existence." The analogy, Darwin noted, was not exact since, in the natural plant and animal world – unlike the human world of agriculture – "there can be no artificial increase in food." Nor in the plant and animal world is there exercised that "prudential restraint from marriage" which Malthus found to exert a moral rein on human population growth.[131]

Maxwell not only made extensive use of analogies but wrote eloquently about their role in science. His discussions of analogy remain today perhaps the best introduction to this subject.[132] One example of his use of analogies occurs in relation to the theory of heat. The "laws of the conduction of heat in uniform media," he wrote, "appear at first sight among the most different in their physical relations from those relating to attractions." Even so, he concluded, we "have only to substitute *source of heat* for *centre of attraction*, *flow of heat* for *accelerating effect of attractions* at any point, and *temperature* for *potential*," and the result is that "the solution of a problem in attractions is transformed into that of a problem of heat." So exact is the formal analogy that "if we knew nothing more than is expressed in the mathematical formulae, there would be nothing to distinguish between the one set of phenomena and the other" – despite the fact that the conduction of heat "is supposed to proceed by an action between contiguous parts of a medium, while the force of attraction is a relation between distant bodies."[133] Having justified this method, Maxwell proceeded to use it in elaborating a mathematical theory of "lines of force" (in Faraday's sense) by making use of an analogy with the "mathematical formalism" of the motion of an incompressible and imponderable fluid.[134]

1.7. RATIONAL MECHANICS AND MARGINALIST ECONOMICS

In considering the role of analogies and similar correlations in the social sciences, two primary areas of nineteenth-century natural science attract our attention. Mathematical physics, consisting of the new rational mechanics plus energy physics, had a profound influence on economics, while the cell theory, together with related aspects of the life sciences, gave new form as well as content to theories of social morphology and behavior.

These two subject-areas illustrate very different aspects of the ways in which social sciences draw on the natural sciences. Rational mechanics with energy physics provided a rich source of conceptual homologues for a rising marginalist (or neoclassical) economics, together with analytical tools such as Lagrangian virtual displacements and Hamiltonian functions, even analogous equations and principles of minimization and maximization. While producing a social science with an external appearance of physics, some of the founders of neoclassical economics wholeheartedly adopted the metaphor of mathematical physics, clearly hoping to give the social science of economics a legitimation (especially in the opinion of natural scientists) and some measure of the value-system of "hard" science.[135] Economists of this school have continued to draw on the body of physics well established by the end of the nineteenth century. Apparently, they have felt little need to encompass within their theoretical structures any later developments such as quantum theory or relativity. An outsider cannot help but be astonished that economics has been affected so little by the later dramatic revolutions in the very subjects – rational mechanics and energy physics – which have provided some of its principal metaphors. For example, there seems to be no current significant economic ripple from the twentieth-century conclusions that the conservation of energy can no longer be considered an independently true principle and that energy itself can no longer be regarded as subject to continuous variation but acts in quantized steps. Perhaps this paradox is to be explained by the judgment of Philip Mirowski and other critics of neoclassical economics that the energy metaphor was only imperfectly understood by the founders, who apparently were not aware that their adopted energy model was flawed because they did not take account of the conservation law.

Ernest Nagel has divided analogies into two classes: "formal" and "substantive." A "substantive" analogy is one in which a theory or a system is patterned on the model of another system which contains known laws.[136] Examples are the kinetic theory of gases (patterned on the known laws of the interaction of elastic spheres such as billiard balls), electron theory (in which the analogy is with macroscopic electrostatically charged bodies), and atomic structure (the model is the solar system). The other type of analogy is "formal," based on a structure of abstract relationships rather than a "more or less visualizable set of elements." An example would be the analogy proposed by J. C. Maxwell based on the isomorphism of the laws of gravitation theory and the laws of heat conduction.[137] Neoclassical economics illustrates the use of such formal analogies.

This kind of example from economics, however, goes far beyond a mere creative transfer of concepts and principles, mathematical expressions, and other tools of the arsenal of mathematical physics. Economics, and to some degree the other social sciences, may illustrate a thesis of Jevons that analogy "leads us to discover regions of one science yet undeveloped, to which the key is furnished by the corresponding truths in the other science."[138] To make this sentiment universally valid, we should enlarge Jevons's "corresponding truths" by adding methods and formal techniques (e.g., equations).

Economics and mathematical physics seem at first sight to be extremely different. Economics deals with such human and moral or ethical factors as greed, profit, cost, value, utility, need, and good. These topics appear to be worlds apart from such abstractions as force, field, distance, speed, and kinetic and potential energy; they appear to be free of affect and seem to lend themselves "naturally" to mathematical treatment. But analogies between very different subjects are not unusual in the history of science: "No two sciences might seem at first sight more different in their subject matter than geometry and algebra," Jevons wrote, since one deals with "forms in space" (circles, squares, triangles, parallelograms, . . .) and the other with abstract "symbols and numbers."[139] Yet, as Jevons pointed out, a crucial step in the development of modern mathematics was the recognition of analogies between these two branches of mathematics. He described Descartes's great breakthrough as a demonstration of a "most general kind," that equations may be represented by curves or figures in space and vice versa and "that every bend, point, cusp or other peculiarity in the curve indicates some peculiarities in the equation." Jevons found it "impossible to describe in any adequate manner the importance of this discovery."[140]

This kind of analogy occurs frequently in the social sciences, notably in economics. In his *Theory of Political Economy*, Jevons took note of the "objections made to the general character of the [differential] equations" which he had employed, defending his position by making an analogy between economics and physics, declaring that economics is similar to physics insofar as "the equations employed do not differ in general character from those which are really treated in many branches of physical science."[141] The example he chose to develop was the use of the principle of virtual velocities (or virtual displacements) applied to the lever, where there is a homology of equations, that is, the equations for the case of the lever "have exactly the forms of the equations [in economics]." He even introduced a diagram in order to "put this analogy of the theories of exchange and of the lever in the clearest possible light."[142] This same kind of analogy of theories was invoked by Léon Walras in an article on analogy, "Economique et mécanique," published in 1909. Here Walras argued that identical differential equations appear in his analysis of economics and in two examples from mathematical physics: the equilibrium of a lever and the motion of planets according to gravitational celestial mechanics.[143] Claude Ménard has described Walras's text on "Economics and Mechanics" as a term-by-term comparison of the proportion between *rareté* (scarcity, i.e., marginal utility) and value – which is the basis of the theorem of maximum satisfaction – with the equation of maximal energy from rational mechanics. In addition, Ménard indicates that Walras' law, defining the properties of general equilibrium in relation to the marketing of goods, services, and money, relies on the example of uniformly accelerated motion from celestial mechanics and invokes equations containing mass and acceleration.[144]

Vilfredo Pareto was writing as an economist when he invoked a similar "formal" homology in the example of "the equations which determine [economic] equilibrium." On seeing these equations, he wrote, a writer trained in mathematical physics (as he was) would observe, "These equations do not seem new to me; I know them well, they are old friends. They are the equations of rational mechanics." Because the equations are the same, he concluded, "pure economics is a sort of mechanics or akin to mechanics."[145]

Pareto envisioned a double role for mathematics in economics and more generally in social science. Mathematics, he believed, provides a means of analogically transferring the basic equations of physics to economics. Mathematics also serves as a primary tool for dealing with such problems as the "mutual dependence of social phenomena" in conditions of equilibrium; here mathematical analysis enables us to make precise "how the variations of any one of these [conditions] influence the others," an assignment in which "we really need to have *all* the conditions of the equilibrium." In the "existing state of our knowledge," he noted, only mathematical analysis can "tell us if this requirement is observed."[146]

This led Pareto to some remarks on the proper role of analogies and the dangers of using them in social science. Since "the human intellect proceeds from the known to the unknown," he wrote, we can make progress in our thinking by basing our ideas of an area of the "unknown" on analogies drawn from an area of the "known." For example, "extensive knowledge of the equilibrium of a material system," helps us to "gain a conception of economic equilibrium" and this in turn "can help us to form an idea of social equilibrium." He warned, however, that in "such reasoning by analogy there is . . . a pitfall to be avoided." That is, the use of analogies "is legitimate, and perhaps highly useful, as long as what is involved is only the elucidation of the sense of a given proposition." We are led into grave errors, however, if we try to use analogies to prove a proposition or even "establish a presumption in its favour." Analogies, he added, serve primarily to clarify the meaning of propositions.[147]

Philip Mirowski has devoted a good part of his book, *More Heat Than Light*, to an argument that such figures as Jevons, Walras, Edgeworth, Fisher, and Pareto – all leading architects of the Marginalist Revolution – based their economics on, or at least associated it with, the mathematics of a specific subset of physics: post-Newtonian rational mechanics (i.e., incorporating principles of Lagrange and Laplace plus the methods of Hamilton) combined with the doctrines of energy. There was thus conceived, on the level of metaphor, a correspondence between economics and physics. And even before the marginalist school of economics had come into being there were expressions of hope that economics might become a true or exact science on the model of mathematical physics. In 1875 this position was expressed clearly by J. E. Cairnes: "Political Economy is as well entitled to be considered a 'positive science' as any of those physical sciences to which this name is commonly applied." The principles of economics, he asserted, are "identical" in character "with that of the physical principles which are deduced from the laws of gravitation and motion."[148] For Jevons, the emerging new economics was regarded as making use of concepts which were direct homologues of physical concepts. With a sense of security coming from the use of equations homologous to those in physics, the new economics assumed the metaphor of rational mechanics and its great founder Isaac Newton, including scientific dignity, precision, esteem, and the whole value system. It is difficult for us today to imagine or reconstruct the veneration in which nineteenth-century scientists held Isaac Newton and his law of universal gravity, but we may gain a hint of the awe inspired by Newton and his law by considering a moment in the life of Charles Kingsley. In 1860, when his son had just died and when it seemed to him that all his foundations of faith were crumbling, the bereaved parent wrote to Thomas H. Huxley: "I know what I mean when I say I believe in the law of the inverse square, and I will not rest my life and my hopes upon weaker convictions."[149] The Newtonian law of gravity, together with the laws of motion, provided a certainty on which intellectuals could agree.

Some founders of the Marginalist Revolution believed in a homology of concepts in economics and in physics, with the consequence that the laws of one could be directly translated into the other. Jevons, for example, stated *expressis verbis* that the "notion of value is to our science what that of energy is to mechanics." He even adopted directly from Maxwell the technique of dimensional analysis (*L*, *T*, and *M*: length, time and mass) and showed that "the *dimensions of commodity*, regarded merely as a physical quantity, will be *the dimensions of mass*." The homology extended ultimately to Newton's law of gravity when Jevons declared that "utility is an attraction between a wanting being and what is wanted" and is "just" like "the gravitating force of a material body."[150]

In a similar vein, Léon Walras later wrote in his *Elements of Pure Economics* that the use of "mathematics promises to convert pure economics into an exact science," that "mathematical economics will rank with the mathematical sciences of astronomy and mechanics." He concluded that "the pure science of economics is a science which resembles the physico-mathematical sciences in every respect."[151] To put this outlook into perspective, Claude Ménard has argued that Walras "sought justification and guarantee as much as inspiration" in his use of the analogy between economics and rational mechanics. In this regard, Ménard stresses the fact that Walras was "always concerned with scientific legitimacy and despairing of recognition of the value of his work."[152] Moreover, in a scholarly study of Walras's economic ideas in relation to Mirowski's analysis, Albert Jolink denies "strenuously" that Walras's economics theory "slavishly imitates physics." Jolink cites evidence that Walras had little or no understanding of energy physics before 1906, and he concludes that even after 1906 it is doubtful whether

TABLE I. Pareto's Analogies

Mechanical Phenomena	Social Phenomena
Given a certain number of material bodies, the relationships of equilibrium and movement between them are studied, any other properties being excluded from consideration. This gives us a study termed *mechanics*. This science of mechanics is divisible into two others:	Given a society, the relationships created amongst human beings by the production and exchange of wealth are studied, any other properties being excluded from consideration. This gives us a study termed *political economy*. This science of political economy is divisible into two others:
1. The study of material points and inextensible connections leads to the formulation of a pure science – rational pure mechanics, which makes an abstract study of the equilibrium of forces and motion. Its easiest part is the science of equilibrium. D'Alembert's principle enables dynamics to be reduced to a problem of statics.	1. The study of *homo economicus*, of man considered solely in the context of economic forces, leads to the formulation of pure political economy, which makes an abstract study of the manifestations of ophelimity. The only part we are beginning to understand clearly is that dealing with equilibrium. A principle similar to D'Alembert's is applicable to economic systems; but the state of our knowledge on this subject is still very imperfect. Nevertheless, the theory of economic crises provides an example of the study of economic dynamics.
2. Pure mechanics is followed by applied mechanics which approaches a little more closely to reality in its consideration of elastic bodies, extensible connections, friction, etc. Real bodies have properties other than mechanical. Physics studies the properties of light, electricity and heat. Chemistry studies other properties. Thermodynamics, thermochemistry and the like sciences are concerned specifically with certain categories of properties. These sciences all constitute the physico-chemical sciences.	2. Pure political economy is followed by applied political economy which is not concerned exclusively with *homo economicus*, but also considers other human states which approach closer to real man. Men have further characteristics which are the object of study for special sciences, such as the sciences of law, religion, ethics, intellectual development, esthetics, social organisation, and so on. Some of these sciences are in an appreciably advanced state; others are extremely backward. Taken together they constitute the social sciences.

TABLE I. *(Continued)*

Mechanical Phenomena	Social Phenomena
Real bodies with only pure mechanical properties do not exist. Exactly the same error is committed *either* by supposing that in concrete phenomena there exist solely mechanical forces (excluding, for example, chemical forces), *or* by imagining, on the other hand, that a concrete phenomenon can be immune from the laws of pure mechanics.	Real men governed only by motives of pure economics do not exist. Exactly the same error is committed *either* by supposing that in concrete phenomena there exist solely economic motives (excluding, for example, moral forces), *or* by imagining, on the other hand, that a concrete phenomenon can be immune from the laws of pure political economy.
The difference between practice and theory arises precisely from the fact that practice has to take account of a mass of details which theory does not deal with. The relative importance of primary and secondary phenomena will differ according to whether the viewpoint is that of science or of a practical operation. From time to time, attempts are made to synthesise all the phenomena. For example, it is held that all phenomena can be ascribed to:	
The attraction of atoms. The attempt has been made to reduce to unity all physical and chemical forces.	Utility, of which ophelimity is only a type. The attempt has been made to find the explanation of all phenomena in *evolution*.

"Walras had any understanding at all concerning a proto-energetic metaphor."[153] In short, for Walras the physical analogues served more as a means of later legitimation of his economics than as a primary instrument of discovery. Yet there can be no doubt that Walras wished to associate his economics with mathematical physics.

Pareto was equally convinced that the "equilibrium of an economic system offers striking similarities with that of a mechanical system," but he was aware that there are special pitfalls for those who study political economy without "a knowledge of pure mechanics." Firm in his conviction that an analysis of a mechanical system is of the greatest help in giving "a clear idea of the equilibrium of an economic system," he drew up a table (printed here as Table I) for "those who have not studied pure mechanics" and who will need help in understanding the argument. In this table he placed in parallel columns some major concepts and principles of physical mechanics and their counterparts in economics. He warned, however, that in such a tabulation of "analogies existing between mechanical and social phenomena" the "analogies do not prove anything: they simply serve to elucidate certain concepts which must then be submitted to the criterion of experience."[154]

The extreme of this proposed homology between economics and rational mechanics is found in Irving Fisher's *Mathematical Investigations into the Theory of Value and Prices* (1926). It should be noted that Fisher was rather well trained in mathematics and physics (as Jevons and Walras were not), having been one of the small group of students who worked for their Ph.D. under J. Willard Gibbs. In the style of Pareto, with whom he was in correspondence, Fisher (see Table II) also drew up a table of homologies from physical mechanics and economics. His compilation, however, goes beyond Pareto's to the extent of including not only paired concepts (such as particle and individual; space and commodity; energy and utility) but also the property of being scalar or vector, and his list was extended to include even general principles.

Philip Mirowski found, however, that despite Fisher's parade of dynamical analogies and homologies, he apparently took "most of his analogies . . . from hydrostatics rather than from fields of force." Mirowski notes, in this regard, that in an unpublished essay on "My Economic Endeavors" Fisher boasted of having pioneered in "hydrostatic and other mechanical analogies." Mirowski has presented a critique of Fisher's table, beginning with the "incorrect" identification "of a particle with an individual." Like other "neoclassical economists," Fisher – according to Mirowski's thesis – made a serious blunder in not appreciating the principle of conservation of energy, which would imply for an economic system "that the sum of total expenditure and the sum of total utility in a closed trading system must be equal to a constant." Mirowski argues that Fisher's general failure to carry the physical analogy to its logical conclusion – that is, to take cognizance of the conservation law – was a logical fault that came from an incomplete understanding of the physics metaphor of energy and field that lies at the very foundation of neoclassical economics.[155] It must be admitted, however, that all economists do not accept this radical critique.[156]

One of the difficulties in using analogies, whether in the natural sciences or in the social sciences, is that there may be more than one analogy for the same problem. The problem of multiple analogies, along with the concomitant need for a decision concerning which one to choose, has long plagued the social sciences. It arose in a dramatic fashion in 1898 in Alfred Marshall's discussion of "Mechanical and Biological

TABLE II. Fisher's Analogies

Mechanics	Economics
a particle	an individual
space	commodity
force	marginal utility or disutility
work	disutility
energy	utility
work or energy = force × space	utility = marginal utility × commodity
force is a vector	marginal utility is a vector
forces are added by vector addition	marginal utilities are added by vector addition
work and energy are scalars	disutility and utility are scalars
The total energy may be defined as the integral with respect to impelling forces.	The total utility enjoyed by the individual is the like integral with respect to marginal utilities.
Equilibrium will be where net energy (energy minus work) is maximum; or equilibrium will be where impelling and resisting forces along each axis will be equal.	Equilibrium will be where gain (utility minus disutility) is maximum; or equilibrium will be where marginal utility and marginal disutility along each axis will be equal.
If total energy is subtracted from total work instead of vice versa the difference is "potential" and is a minimum.	If total utility is subtracted from total disutility instead of vice versa the difference may be called "loss" and is minimum.

Analogies in Economics."[157] After a discussion of dynamics and statics in relation to economics, Marshall – who was well trained in physics and mathematics – expressed deep skepticism about the analogy with physics. He concluded that while "there is a fairly close analogy between the earlier stages of economic reasoning and the devices of physical statics," there is not "an equally serviceable analogy between the later stages of economic reasoning and the methods of physical dynamics." At the later stages, he argued, "better analogies are to be got from biology than from physics." Accordingly, "economic reasoning should start on methods analogous to those of physical statics, and should gradually become more biological in tone." This need for shifting analogies was apparently very important for Marshall. Analogies, he wrote, "may help one into the saddle, but are encumbrances on a long journey." That is, it is "well to know when to introduce them, it is even better to know when to stop them off." He concluded that "in the later stages of economics, when we are approaching nearly to the conditions of life, biological analogies are to be preferred to mechanical."[158] On the title page of Marshall's *Principles of Economics* there is a biological apothegm taken directly from Darwin's *Origin of Species*: "Natura non facit saltum."

1.8. BIOLOGICAL THEORY AND SOCIAL THEORY

The situation with respect to the "organismic" theories of society is quite different from that of marginalist or neoclassical economics. The sociologists, unlike the economists, gloried in revealing the sources of their analogies and homologies and other comparisons and correlations and in showing how current their biological knowledge was. They even went to the extent of inserting in their sociology biological tutorials on the latest development. In three cases examined below, those of Lilienfeld, Schäffle, and Worms,[159] we can see the joy and satisfaction derived from using the latest findings in biology. This trio of thinkers shared the historical recognition that the cell theory had brought the life sciences to a state of maturity – a conclusion which led to hopes that the use of the cell theory would produce a similar effect in sociology. One can trace in their sociological works the successive ideas of von Baer on embryological development and the increase of complexity as a part of development, the doctrine of Milne-Edwards and others on division of labor in relation to cell function and structure, Virchow's cellular pathology, and the new ideas relating to the germ theory of disease.

Organismic sociology and marginalist economics differ even further in a number of fundamental respects. Some founders of the Marginalist Revolution (e.g., Jevons and Walras) were deficient in their actual understanding of the mathematical physics which they claimed their subject to be emulating, while the proponents of the organismic theories of society had a sound grasp of biological principles – perhaps an easier assignment than to understand physical principles. The greatest differences between the two groups, however, is that neoclassical economics still flourishes as a dominant school of thought, whereas the organismic theories of society have largely withered away and may even seem ridiculous to today's reader. As a result, the preceding material on economics appears to be a study of the founding period of today's thought, whereas the ideas of the organismic sociologists seem so extravagant that many historical examinations of the writers of this school end up as total disparagements.[160]

Why was there in the late nineteenth century so vigorous a school of social thought based on an exact parallel with the life sciences? In order to understand why, we must take into account the great achievements of the biological sciences, together with the extraordinary successes of medicine, in the nineteenth century. This century witnessed tremendous advances such as the cell theory and the theory of evolution plus developments in embryology, physiology, and morphology that completely transformed the subject. The new science of microbiology had not only opened up an exciting new realm of biology but at last provided medicine with a knowledge of the causes of contagious diseases and even showed the way to prevent or to cure some of them. Great new prospects seemed in store for the life sciences: conquest of yet additional diseases, finding the key to the origins of life, understanding the processes of heredity, and much more. By contrast, physicists were sounding the gloomier message of making more exact measurements of the constants of nature, even expressing a conviction that the future was to be found in the next decimal place. We may easily understand why many social scientists of the late nineteenth century could believe that a new great age of biology was taking the place of the older great age of physics. This point of view was expressed dramatically by the economist Alfred Marshall in his "Inaugural Lecture" at Cambridge University in 1885. "At the beginning of the nineteenth century," he said, "the mathematico-physical group of sciences was in the ascendant." But now "the speculations of biology [have] made a great stride forwards." The discoveries in biology, he continued, now attract "the attention of all men as those of physics had done in earlier years." The result was that the "moral and historical sciences of the day have . . . changed their tone, and Economics has shared in the general movement."[161]

Additionally, not only were social scientists impressed by the achievements of biological science, but many sociologists were convinced, as Comte had taught explicitly, that because sociology deals with human behavior it must be a science very close to, or very much like, biology. There is, accordingly, no reason to wonder why the organismic sociologists chose to construct a science in emulation of biology, and as historians we may focus our attention on the degree to which they were successful in finding relevant analogues and homologues for producing a biological science of society. The important bio-medical subjects used by sociologists included the cell theory; the new embryology; the physiology centering on the *milieu intérieure*; cellular pathology; the germ theory of disease; new theories concerning psychological disorders, notably hysteria;[162] and, of course, the theories of evolution.[163]

Most organicists traced a lineage that went back to Auguste Comte. Although he is usually thought of primarily in the context of social physics, Comte also made extensive use of the organismic metaphor, drawing heavily on physiology and pathology. In his *Course of Positive Philosophy* he clearly set forth the important point of view that social disturbances should be regarded as pathological cases, being, "in the social body, exactly analogous to diseases in the individual organism." Comte held the extreme position that in the development of biological science, "pathological cases are the true equivalent of pure experimentation." It followed that the study of social pathology should provide the equivalent of social experiment, something which he was aware can never occur to the same degree and kind as in physics or chemistry.

Comte esteemed and drew heavily on the ideas of Broussais, one of the great reformers of medicine; in his *System of Positive Polity* (1848–1854), Comte wrote of " . . . the admirable axiom of Broussais" which "destroys the old absolute distinction between health and disease." Between these extreme limits, Comte added, "we may always find a multitude of intermediate stages, not merely imaginary, but perfectly real, and together forming an almost insensible chain of delicate gradations."[164] Broussais taught Comte that pathology, "the study of malady, is the way to understand the healthy state." Primarily it was his "principle of continuity" which guided Comte's own analysis: "that the phenomena of the pathological state are a simple prolongation of the phenomena of the normal state, beyond the ordinary limits of variation." Until now, Comte declared, no one had drawn the analogy between physiological and social pathology, no one had ever applied "this principle to intellectual and moral [i.e., social] phenomena."[165]

At the century's end, in 1896, the American cytologist Edmund Beecher Wilson boldly declared the cell theory to be the second great generalization made by biology, the first having been organic evolution.[166] In retrospect, insofar as social theory or social science is concerned, the cell theory seems to have been at least of equal importance with the Darwinian evolutionary theory. It is easy to see why the cell theory and its sequelae had so great an impact on social science. As many were quick to observe, the concept of a natural organism as an organized system of living cells provided a new scientific foundation for an organismic concept of society. The cells resemble the individual members of human society in the degree to which each cell has a life of its own while the lives of all the cells are linked together. Additionally, the cells in the animal or human body exhibit the principle of the physiological division of labor, since each type of cell has a structure especially adapted for its function within the organism: nerve cells to transmit messages or commands, liver cells to produce bile, and so on.[167] This principle became central to the biological thought of Milne-Edwards and others and from them passed at several removes to Emile Durkheim, who applied it within a sociological framework in his major doctoral dissertation. Furthermore, cells are grouped together into functional units (tissues, organs) just as human individuals are organized into social units. Even the distribution or circulation of nutriments and the discharge of waste products could be seen analogically in natural bodies composed of cells and in social bodies composed of humans.

The analogy ultimately broke down because, although each cell has a life of its own, no body-cell can survive on its own apart from the parent body-matrix. Remove a muscle cell and it will, of and by itself, quickly die. Additionally, there is the problem of "will," which exists in each human being in society and which has no counterpart in individual cells.[168] Furthermore, the animal body differs from the social organism in having a relatively short and determinate lift-span and in exhibiting a regular sequence of universally accepted and recognized symptoms of decay and old age.[169] Nevertheless, the perceived similarities continued to be important in social science.

The significance of the cell theory for a science of society was enhanced by the embryological discoveries of Karl Ernst von Baer and his successors. The recognition of the stages of development of the embryo by cell division from a single cell, and the subsequent elaboration of organ and tissues, suggested a similar sequence of social organization, starting from a single mother (as the original cell) and, by subsequent multiplication, accompanied by grouping of individuals (similar to the grouping of cells), forming family units, then tribes, and eventually countries.[170]

Of special importance for social scientists was von Baer's principle that the stages of development form a sequence characterized by a transition from simplicity to greater and greater complexity. This was similar to the discovery that extinct and living forms of animals could be ordered in an ascending scale of development in which there was an increasing degree of complexity.[171] In its most complete form, this result became encapsulated in the famous "biogenetic law," that "ontogeny recapitulates phylogeny," which preceded Darwin's theory of evolution and was found to be conformable to either Darwinian or Lamarckian evolution.[172]

Social scientists such as Herbert Spencer[173] drew upon von Baer's results to formulate an analogous developmental theory for society, conceived to have gone through a regular sequence from infancy to old age just like the development of mankind from savagery to civilization.[174] There would thus be a general law of evolution that applies to the development of animals from earliest times, to the development of the embryo, to the development of civilization, and to the development of societies. In a piece first published in 1864,[175] Spencer explained that his thought had been profoundly influenced by "the truth that all organic development is a change from a state of homogeneity to a state of heterogeneity" and that it was von Baer who had given this truth "a definite shape." Incorporating von Baer's "formula" into his own "beliefs in evolutions of various orders," Spencer had further extended and modified both von Baer's concept and his own insights, thus involving his thought in a process "of continuous development, set up by the addition of von Baer's law to a number of ideas that were in harmony with it."[176]

An aspect of the cell theory which had special importance for nineteenth-century organismic sociology, and especially for consideration of the social analogues of the cell theory, was introduced by Rudolf Virchow's doctrine of "cellular pathology" (1858).[177] This doctrine held that all pathological conditions of the human body could be attributed to a state of degeneration or a condition of abnormal activity of some individual constituent cell or cells. Thus Virchow transformed thinking about the body as a whole to thinking about conditions of the fundamental biological units of which the body is composed. One important consequence of Virchow's doctrine was that pathological conditions were seen as merely extremes of the normal rather than as different in kind. In the present context his ideas are especially interesting also because the biologist himself stressed the similarities between biological phenomena and sociological phenomena. According to Virchow, just as a tree constitutes a mass arranged in a definite manner, in which, in every single part, in the leaves as in the root, in the trunk as in the blossom, cells are discovered to be the ultimate elements, so is it also with the forms of animal life. *Every animal presents itself as a sum of vital unities*, every one of which manifests all the characteristics of life. The characteristics and unity of life cannot be limited to any one particular spot in a highly developed organism (for example, to the brain of man), but are to be found only in the definite, constantly recurring structure, which every individual element displays. Hence it follows that the structural composition of a body of considerable size, a so-called individual, always represents a kind of social arrangement of parts, an arrangement of a social kind, in which a number of individual existences are mutually dependent, but in such a way, that every element has its own special action, and, even though it derives its stimulus to activity from other parts, yet alone effects the actual performance of its duties.[178]

Believing that all plants and animals are aggregates of cells as the fundamental life-units, Virchow concluded that all structural and functional properties of organisms are determined by relations among individual cells.[179] In referring to the cells as providing the "living organism" with a "multiplicity of vital foci," Virchow explained that every organism

is a free state of individuals with equal rights though not with equal endowments, which keeps together because the individuals are dependent upon one another and because there are certain centers of organization with whose integrity the single parts cannot receive their necessary supply of healthful nourishing material.[180]

As Owsei Temkin has indicated, "the metaphor of the cell state for Virchow was not a mere manner of speech, but an integral part of his biological theory."[181] Here we note a striking example of the use of social concepts in the thought of a biologist.

Virchow provided a direct model for such social scientists as Lilienfeld and Schäffle. Of particular importance in this respect is Paul von Lilienfeld's[182] *Social Pathology* (1896), which must be read in the light of his five-volume opus, *Thoughts on the Social Science of the Future* (1873–1881). In the earlier work, at the beginning of the first volume, *Human Society as a Real Organism*, Lilienfeld issued his challenge:

Human society is, like natural organisms, a real being, is nothing more than a continuation of nature, is only a higher expression of the same forces that underlie all natural phenomena: This is the assignment, this is the thesis, which the author has set himself to accomplish and to prove.[183]

In *Social Pathology* he continued the challenge by asking how the study of society could be made truly scientific and by giving the solution which he felt he had demonstrated in the *Thoughts*:

> The condition *sine qua non* by which sociology may be raised to the rank of a positive science and by which the inductive method may be applied to it is . . . the conception of human society in its character as a real living organism, composed of cells as are the individual organisms of nature.[184]

He went on to identify the cellular structure of society: "Social cells are human individuals forming first the family, then the clan, the tribe, the nation," and finally, at present, the state, and probably, in the future, humanity as "a great organic whole."[185]

In developing the "physiology and morphology" of the human social organism, Lilienfeld gave primacy to a "constituent factor of every human association, beginning with the family and going up to the state and the whole human race." This is the "nervous system, the source of all social action."[186] He argued that "the intercellular substance of individual organisms" corresponds in the social organism to "the wealth produced, exchanged, and consumed." He provided evidence to support his conclusion that any action of the social nervous system on this ambient milieu is very much like the physiological action of individual organisms endowed with nervous systems. He further insisted that in the social organism there are specific nerve energies just as in natural organisms, "not only in a figurative sense, but really."[187]

Lilienfeld hailed Virchow's "cellular pathology" as "one of the most striking conquests of modern science." From Virchow he had learned that "every pathological state of the human body derives from a degeneration or an abnormal activity of the simple cell, as the elementary anatomical unit from which every organism is constructed."[188] Furthermore, Lilienfeld wrote, Virchow had taught that "there is no essential and absolute difference between the normal state and the pathological state of an organism." In a "deviation from the normal state," Lilienfeld declared (on Virchow's authority), "a cell or a group of cells manifests an activity outside the necessary time, outside the necessary place, or outside the limits of excitation prescribed by the normal state."[189]

> As every individual disease derives from a pathological state of the cell, likewise every social disease has its cause in a degeneration or abnormal action of the individual who constitutes the elementary anatomical unit of the social organism. Likewise, a society attacked by disease does not present a state essentially different from that of a normal society. The pathological state consists only in the manifestation by an individual or a group of individuals of an activity that is untimely or is out of place or indicates over-excitement or lack of energy.[190]

While there is no doubt that Virchow's "cellular pathology" revolutionized medicine, it had a fundamental weakness in that it did not take account of communicable diseases, a topic illuminated by the germ theory of disease. Lilienfeld was aware of this deficiency and accordingly supplemented Virchow's analysis by introducing the discoveries concerning the germ of disease. "It has now been proved," he wrote, "that to each disease there corresponds a specific bacillus." As we would expect, Lilienfeld identified certain social diseases as similarly caused by "specific parasites." The "social organism is infested with economic, juridical, and political parasites," subdivided "into several classes and species, each one of which corresponds to a special disease of the social organism."[191]

Lilienfeld argued that there are even more fundamental and remarkable connections between living organisms and society. He held that "organic nature itself presents three degrees of development and perfection." The first is that plants cannot move autonomously, either together as a whole or separately as parts. The second: animals can move freely, but only as individuals, that is, as parts. But, third, a "social aggregate" can move freely both as a whole and in its parts. Thus "it is only in human society that nature realizes in its fullness the highest degree of organic life: the autonomy of the same individual organism in the parts and in the whole."[192]

The viewpoint of Albert Eberhard Friedrich Schäffle (1831–1903) is stated unambiguously in the title of his book, *The Structure and Life of the Social Body* (1875–1878), and especially in its subtitle, which declares it to be an "encyclopedic sketch of a real anatomy, physiology, and psychology of human society" in which the "national economy [is] considered as the social process of digestion."[193] Schäffle was aware that the correspondence between human society and animal bodies is imperfect since the ties between humans derive from the mind and are not physical. "No uninterrupted occupancy of space," he wrote, is observed in the substance of society, in contrast to the "organic body," in which "cells and intercellular parts form a solid object."[194] That is, in the social body there are no physical forces such as "cohesion, adhesion, or chemical affinity" to "effect coherence and co-ordination," but rather there are "mental forces" which establish "spiritual and bodily connection and co-operation between spatially separated elements."[195] Because of the presence of such statements, Schäffle can be said to have differed from Lilienfeld, as René Worms noted, in that he regarded society only as an "organized" entity, whereas Lilienfeld believed society to be a "fully organic system," an "*organisme concrète*."[196] Yet, as Stark has pointed out, although Schäffle concedes that it is impossible to make a strict comparison between physical and intellectual or psychic entities, nevertheless he insists that there is no "essential difference between social and organic tissues."[197] Accordingly the greater part of Schäffle's four enormous volumes consists of comparisons of the social body and the physiological body.

Like Lilienfeld, Schäffle held that the fundamental unit of society must be the equivalent of the biological cell: his starting point was that "the simplest elements of the bodies of the higher species of plants and animals" are "cells and the intercellular substances interspersed between them."[198] He concluded that "the family has all the traits of the tissue cell," that "every fundamental trait of the structure and function of the organic cell is repeated here."[199] After noting many similarities between organic and social bodies, Schäffle decided that "in all social organs" there is a "tissue which looks after the intake and outflow of the materials of regeneration and nutrition from and to the channels of economic production and circulation and secure[s] a normal digestion on the part of all the elements of the organ or organic part concerned." This "tissue" or social institution is "the household." Schäffle thus made a comparison between the household "and the capillary tissues of the animal body":

> The great social digestive apparatus, in other words, the national economy, the production and circulation of commodities, leads in the end to as many households as the body social has organs and every organ independent tissues and tissue elements.[200]

He found a perfect parallel between the vegetable and animal processes of digestion and the processes of production in human societies, even to the point of believing that "primary production marks the starting point" and "the ejection of human corpses and material waste the end point of external social digestion."[201]

Schäffle found a close resemblance between the streets, roads, buildings, and other constructional elements of the human living space and "the bone and gristle tissue" of animals. As Stark has shown, Schäffle even managed "to find homologues to such protective tissues of the animal body as hair, nails, and horny skin," whose social counterparts (Schäffle himself says they occur "analogically in the body social") are "roofs, coverings, wrappings, fences, walls, clothes, even picture-frames and book-covers. . . ."[202] He also found a likeness between the "collective property [of the social organism] in literature, works of art, roads, transport undertakings, defensive institutions, institutional buildings, and public works equipment" and "the circulating, solvent and protective material which serves the organic body by means of liquidity, softness, elasticity, the chemical counterbalancing of destructive affinities and in other ways." One can hardly overlook, he wrote, the close "analogy of an organic mass of property of the cells in the intercellular materials" and "the social institution of a mass of property in tools, coverings, means of transportation and collective arrangements of all sorts."[203]

René Worms,[204] who devoted a whole treatise to *Organism and Society* (1896), also went beyond merely stating that society is comparable to an organism or is only the analogue of an organism. Like Lilienfeld and Schäffle, he declared that society "constitutes an organism, with something essential in addition." His goal was to go a step beyond Schäffle, "who passes in France as one of the most intransigent partisans of our theory," and Lilienfeld, whom he criticized for having made too many observations "more ingenious than certain," as well as observations "more piquant than decisive." Society, Worms declared, must be an organism because it is a collection of organized living entities fulfilling all the defining requirements of a biological system. He believed that his organismic view of society illustrated Claude Bernard's definition that "vital properties are in reality only in the living cells, all the rest is arrangement and mechanism."[205]

In his detailed development of an organismic sociology, Worms, like his predecessors in this field, introduced extensive biological tutorials and drew heavily on recent work in histology, cellular morphology and physiology, and pathology. A major source was Spencer's *Principles of Sociology*, a work continually cited by him with the highest respect. Proceeding in the manner of a biological analyst, Worms began his treatise with a discussion of the anatomy of societies, then turned to social physiology, and concluded with social pathology.

In his later *Philosophy of the Social Sciences* (1903), Worms confessed that several years after publishing his *Organism and Society* he had been led, "by personal reflection and by discussion," to moderate the "intransigence of my earlier conclusions." Primarily, he admitted to having underestimated the "true value of the individual by making him a simple cell in the social body" and by believing him to be "chained by physio-biological laws." He had accordingly neglected the power of "free will" and the degree to which man is regulated by "the laws which he had given to himself" and the "contracts he made."[206]

In discussing the alterations of his point of view, he gave importance to the ideas of Ernest Haeckel and to the recent discovery of the relative discontinuities among cerebral cells, which he said is "the glory of Golgi and Ramón y Cajal." He also pointed to E. Metchnikoff's research on phagocytes, which had indicated new aspects of cells, even to the extent of suggesting that cells exercise an art or even a science in defending themselves against their enemies. In short, Worms said, his sociological critics had undermined his earlier too-simplistic view of the social organism, while the advances of bio-medical science had been radically altering the scientific base on which the earlier organismic sociology was founded, primarily by enlarging our knowledge and understanding of the life and functions of the cells of which living organisms are made.[207] In the present context, the details of the differences between the two treatises are of less interest than the fact that in each of these works the social parallels drawn from the life sciences were direct reflections of the changing current state of biological and medical knowledge.

In addition to discussing Lilienfeld, Schäffle, and Worms, I might add here a detailed treatment of Herbert Spencer if his use of analogies were not the subject of a later chapter in this book. Spencer is a challenging figure in the history of ideas. Probably one of the most influential thinkers of the nineteenth century, at least in social thought, he is often disparaged today.[208] For example, the author of a recent article in *Nature*, Jonathan Howard, proposed that "books on the history of evolution" should be "avoided in direct proportion to the number of references they make to Spencer."[209] The ambiguity concerning Spencer is perhaps symbolized by the frequently cited quotation from Charles Darwin's *Autobiography* to the effect that Spencer's *Principles of Biology* made him feel that Spencer was "about a dozen times my superior" and that he believed that Spencer would come to be considered the equal of Descartes and Leibniz. As J. W. Burrow wryly remarked, Darwin rather spoiled the effect by adding "about whom, however, I know very little."[210]

Spencer's extravagant use of analogies has already been noted. He not only employed organic analogies, however, but also would suddenly shift to mechanical analogies, rather than keeping to one or the other. As

Peel observed, Spencer "can speak in terms drawn from physics, and go on: 'changing the illustration and regarding society as an organism.'"[211] For example, noting that "in an animal organism, the soft parts determine the forms of the hard ones," he concluded by analogy that "in the social organism the seemingly fixed framework of laws and institutions is moulded by the seemingly forceless thing – character." This conclusion is soon afterwards restated in an engineering analogy, in which an institution is said to resemble a building because its structure is determined by "the strength of the materials" rather than "the ingenuity of its design."[212]

Toward the end of his life, in his autobiography, Spencer repudiated emphatically "the belief that there is any special analogy between the social organism and the human organism." He concluded:

> Though, in foregoing chapters, comparisons of social structures and functions to structures and functions in the human body, have in many cases been made, they have been made only because structures and functions in the human body furnish the most familiar illustrations of structures and functions in general. . . . Community in the fundamental principles of organization is the only community asserted.[213]

On this score we may agree with Spencer's biographer that "there is a certain incompatibility, if not inconsistency, between Spencer's awareness of the proper logical bounds of the comparison, and the evident pleasure he took in picturesque and striking parallels."[214]

Today's sociological literature exhibits an almost universal disdain for organismic sociology,[215] usually without attempting to find out whether this school of thought, admittedly influential in its own time, has left us a permanent legacy. One important contribution of the organicists has been to transfer to social thinking at large some of the concepts and principles developed in medical science. Comte, Lilienfeld, Schäffle, Worms, and others stressed the medical concepts of normal and pathological. They advocated the important principle (adopted in the first instance by Comte from Broussais) that normal and pathological social states should not be considered wholly different types of conditions but rather extreme stages of a single type of condition. The insistence of these writers on this medical analogy is echoed today in such phrases as "a healthy society." And when the organicists even drew on Virchow's medical pathology and sought for social analogues of the germ theory of disease, they were working in the same mode as those who, in our time, have adopted concepts of psychoanalysis to sociological analysis.

One of Michel Foucault's many prescient though startling conclusions was that only "ignorance" has caused sociologists to seek their origins in Montesquieu and Comte, whereas they should have recognized that "sociological knowledge is formed in practices like those of the doctors."[216] This organicist theme of an analogy between sociology and medicine is a feature of older, historically oriented textbooks such as *An Introduction to the Study of Sociology* by Small and Vincent,[217] and the theme appears explicitly in more recent times, as Bryan Turner found out, in Louis Wirth's notion (in 1931) of "clinical sociology" and "sociological clinics."[218] In 1935 L. J. Henderson put forth the case that sociologists should adopt the analogy of clinical medicine (and even its techniques) and anticipated Foucault in conceiving (in 1936) "the practice of medicine as applied sociology."[219]

To organic sociologists it seemed an obvious analogical conclusion from medicine that social ills or diseases are caused by ailing individuals, just as Virchow taught that medical disorders should be reduced to a pathological condition in individual cells. Even earlier, in the eighteenth century, there was a strong current of thought linking individual health or well-being to the health of society. Utopians such as Condorcet drew an analogy between the eventual achievement of a perfect condition of individual health and the creation of a perfect society, predicting a time when people would become so healthy and long-lived that death would, as he wrote, become a "curious accident."[220] But the studies of Malthus on population took a decidedly different turn and showed that this analogy between health of the individual and the health of society might be too facile. Malthus demonstrated with grim examples that health and natural vigor in procreating could be a cause of social ills and diseases, producing a "power of population" restrained only by misery or vice.[221] The effect of human health and the healthy, natural "desire and power of generation" (as Hume described this human drive[222]) would naturally lead to poverty, hunger, and misery, Malthus argued, because any possible increase in food supply (limited to an arithmetic ratio) could never keep up with the increase of population (in a geometric or exponential ratio). The widespread influence of Malthus and the discussions of the social implications of population studies indicate that biological considerations are not essentially foreign to sociology and may give us a measure of the importance and originality of the organicist sociologist who explored a different and valuable set of analogies.

1.9. INCORRECT SCIENCE, IMPERFECT REPLICATION AND THE TRANSFORMATION OF SCIENTIFIC IDEAS

In the use of the natural sciences for the advancement of the social sciences, it may happen that the science being applied is simply wrong. A conspicuous example is provided by the American sociologist Carey, who sought to build a science of society on physical principles centering on Newtonian celestial mechanics. I have mentioned Carey's ideas earlier as an example of mismatched homology, in reference to the concept of mass and the form of his law. Carey also made a grave error in stating the law of universal gravity, wrongly believing that the force between two gravitating masses is inversely proportional to the distance between them rather than inversely proportional to the square of the distance between them.[223] Although this error is obvious to anyone who is even slightly familiar with elementary physics, it has not been noted by Carey's critics.[224] Of course, an argument can be made that Carey's system would not have been any better if he had used the correct Newtonian law. Since he did not develop his subject mathematically, his ignorance of the exact form of the law of gravity may be irrelevant. But such a conclusion condemns Carey's sociology for falsely claiming to be based on Newtonian principles. I strongly doubt whether any sociologist – or other social scientist – would advocate that his or her subject be founded on blatantly erroneous science.

In the application of the natural sciences to the social sciences, errors such as Carey's are not so common as misinterpretations and imperfect replications. An example of a misinterpretation appears in Montesquieu's celebrated *Spirit of the Laws* (1748). In discussing the "principle of monarchy", Montesquieu wrote, "It is with this kind of government as with the system of the universe." That is, "there is a power that constantly repels all bodies from the center, and a power of gravitation that attracts them to it."[225] This notion of a "power of gravitation" that "attracts" all bodies to a center is, of course, Newtonian. But Newton's explanation of the "system of the universe" expressly denied any balance of centripetal and centrifugal forces. Montesquieu had only an imperfect understanding of the Newtonian concept of universal gravitation. In the example under consideration, he shows his essential belief in the older framework of Cartesian physics and balanced forces, into which he tried to introduce a quasi-Newtonian concept that does not fit. There is abundant evidence that Montesquieu remained a Cartesian and never fully grasped the principles of the new Newtonian natural philosophy.[226]

An instructive example of a different sort, at first glance seeming to imply an imperfect replication rather than a misunderstanding, occurs in Adam Smith's *Wealth of Nations* (1776), in the discussion of his celebrated concept of "natural price." Smith wrote that the "natural price" is "the central price, to which the prices of all commodities are continually gravitating."[227] The use of the words "all" and "continually gravitating" invoke Newtonian science and may even suggest that this passage is an instance of Smith's alleged Newtonianism in economics. Unlike Montesquieu, Smith had some understanding of Newtonian scientific principles and, in his essay on the history of astronomy,[228] wrote in glowing terms about Newton's scientific achievements.

Smith's use of gravitation in relation to the natural price differs in one important feature from Newtonian or physical gravitation. A basic axiom of Newton's physics is his third law of motion, that action and reaction are always equal. A consequence of this law is that "all" bodies are not only "gravitating" toward some central body, but are also mutually "gravitating" toward one another. The central body, accordingly, must be "gravitating" toward all other bodies in the system. As a result, for Smith's economics to be a complete and accurate replication of Newton's physical theory of gravity, all prices would have to "gravitate" toward one another and the "natural price" would analogically have to "gravitate" to the "prices of all commodities."

Accordingly, we may all the more admire Smith for having only partially replicated the Newtonian physical concept, for having adapted or transformed the Newtonian physical concept in a way that was of use in economics. Only a brash display of historical Whiggism would fault Smith on the grounds of imperfect replication. The fact is that he was creating a concept for the science of economics and not working on a problem in celestial physics, not pursuing research in the applied physics of gravitation.

Smith's use of a gravitating economic force may serve as a reminder that economics is not an exact clone of physics and that the concepts used in economics need not be exact homologues of those originating in physics. This principle has been stated in a most incisive manner by Claude Ménard, who makes the important point that the successful use of analogies is "not simply a transparent transposition of concepts and methods," that the creative use of analogies always "highlights a difference." He concludes that in every "transfer of concepts" from one domain to another "these concepts take on a life of their own in the reorganized science."[229]

Some recent historical studies of economics seem to be based on a different tacit assumption, namely, that a valid social science must not only be an analogue of the natural sciences but must replicate the natural sciences in every degree of homology of concepts and principles. The history of the natural sciences, however, shows that many of the greatest advances have come not so much from a cloning transfer of ideas from one branch of science to another as from a transformation, from a significant alteration of the original. We may see this process in the way in which Newton forged the concept of inertia as a property of mass, the primary step in the revolution that produced modern rational mechanics. The term "inertia" was introduced into physics by Kepler as part of his argument for the Copernican system. In the pre-Copernican systems, such as those of Aristotle and Ptolemy, the earth was stationary, fixed or immobile at the "center" of the world. Thus, in Aristotelian physics, a terrestrial or "heavy" object is said to fall "naturally" toward the earth's center, which is its "natural place," clearly defined and fixed in space at the center of the immobile earth. For a Copernican, however, since the earth is in continual orbital motion, its center has no fixed or permanent place at the center of the world. There is, therefore, no "natural place," in the old Aristotelian sense, for a falling body to seek. Therefore, Kepler postulated that matter is fundamentally "inert" or is characterized by "inertness" or "inertia." Because matter is inert, it cannot move of and by itself but requires a "vix motrix" or "moving force" for motion to occur. If the "moving force" ceases to act, Kepler concluded, the body will necessarily come to rest then and there, wherever it happens to be. He thus eliminated the anti-Copernican dogma of "natural places."

Newton transformed Kepler's idea, keeping the name which Kepler had introduced. That is, he did not replicate Kepler's idea in his own system of physics. In his transformed concept, there was a very different consequence of the body's "inertness" or "inertia." Whenever there is no externally acting force, Newton wrote in Definition Three and in the First Law of Motion in the *Principia* (1687), a body will persevere in either a state of rest of a "state of motion," that is, uniform motion in a straight line. Newton was consciously aware of the difference between his concept and principle of inertia and Kepler's.

In his personal copy of the *Principia*, he entered the comment that he did not mean by "inertia" Kepler's "force of inertia," by "which bodies tend to rest," but rather a "force of remaining in the same state, whether of resting or of moving."[230]

A somewhat similar transformation of scientific ideas was a feature of Darwin's creation of his theory of evolution. At that time, the geologist Charles Lyell interpreted the paleontological record, marked by successive disappearances of species, in terms of a contest for survival among different species. Charles Darwin, contemplating Lyell's ideas while reading in Malthus, transformed Lyell's concept. Darwin had observed that the individual members of any single species differ from one another in heritable characteristics. Recognizing that certain characteristics were better suited than others for survival in a given environment, Darwin made a radical change in Lyell's idea. Rather than supposing a competition for survival among different species, Darwin proposed that the contest takes place among different individuals of the same species, leading over the course of time to species modification. In making this transformation of Lyell's concept, Darwin introduced into biological thought what is known today as "population thinking," which – according to Ernst Mayr – was one of Darwin's most original and most significant innovations.[231]

Such case histories illustrate the enormous force of the human imagination in transforming an existing concept or principle or theory in the natural sciences. These are not "cautionary tales" of erroneous or imperfect replication, but rather illustrations in detail of the creative process in science at its highest degree. Awareness of such case histories, furthermore, may serve to alert the critical historian to a feature that often appears when a natural scientist or a social scientist makes use of concepts, principles, theories, and methods from another domain. Whether the transfer occurs on the level of analogue, homologue, or metaphor, there is commonly some kind of distortion or transformation that arises from the differences between disparate realms of knowledge. Part of the distortion observed by Ménard, Mirowski, and some other critics of neoclassical economics arises from the "absence of laws of conservation in economics." There is, however, no universal agreement among economists that the omission of a conservation principle so distorts the energy analogy that it constitutes an irreparable fault in the foundations of neoclassical economics.[232] For that matter, a number of economists doubt Mirowski's blatant assertion that neoclassical economists "copied their models mostly term for term and symbol for symbol" from physics. For example, in a review of *More Heat than Light* in the *Journal of Economic Literature*,[233] Hal R. Varian disputes Mirowski's "claim that neoclassical economics is 'incoherent' because of the misappropriation of the energy concept." He also rejects Mirowski's parallel claim "that conservation of energy is an inherent aspect of the physical concept of energy, and that this sort of conservation principle is not valid for utility," so that "utility is not an intellectually coherent concept." Varian's conclusion is that Mirowski has only shown "that utility is not energy."[234]

Such questions of distortion or transformation in the transfer of concepts, laws, principles, and theories are different, however, from simple errors of fact. Carey's social law is not the result of a distortion or a creative, non-orthodox interpretation of Newtonian science. Carey simply made an error in physics; he just did not know the correct gravitational law. Similarly, Montesquieu did not distort Newtonian physics, nor did he omit a significant feature (as was the case for Smith and the mutuality of gravitation or of Walras and conservation); rather he misunderstood or did not know the Newtonian explanation of curved orbital motion. I have mentioned that Carey's sociology would not be in any way different if he had known and used the correct Newtonian law. Similarly, Montesquieu's social and political ideas would not be significantly altered by the substitution of a correct for an incorrect Newtonian explanation; it probably would not make much difference to his system or to the thrust of his argument if the Newtonian references were completely eliminated.

There are, however, many examples of fruitful advances in social thought resulting from transfers in which the original concept or principle may not be fully understood. An example is found in the intertwined history of biological and social thought relating to the principle of division of labor, analyzed by Camille Limoges (in Chapter 9 below). Indeed, it is generally known among social scientists that misinterpretations often lead to very fruitful results, even when the source is another social science. A celebrated example from political science is the doctrine of the separation of powers, a central feature of the form of government adopted in the Constitution of the United States. One direct source for this principle, as A. Lawrence Lowell has documented, is a misreading of the ideas of Montesquieu.[235]

1.10. INAPPROPRIATE OR USELESS ANALOGIES

All analogies are not equally useful. The extreme case occurs when an analogy is so inappropriate as to have no utility for social science. This is not a matter of personal judgment, but a fact of history. Two analogies that have frequently been used in considering the state or society have proved to be inappropriate. One is taken from the biological or life sciences, the other from the physical sciences. One is part of the organismic analogy of the state as the body politic; the other is the Newtonian analogy of the state or society as a physical system. We have seen how in our own century Walter Cannon conceived that his own researches might give the organismic analogy new life. But Cannon did not provide any significant new insights into the theory of society. Nor have any successors to my knowledge made use of his general analogy in a fruitful way. The only possible conclusion is that, in the form presented, the analogy has proved to be inappropriate for the development of sociological knowledge or understanding. If an analogy does not provide a gauge of the validity of a social theory or system or concept or does not introduce some new insight into the social science, then the analogy, being of no use to the social science, must be deemed inappropriate.[236]

The notion that gravitational cosmology or the Newtonian system of the world could provide an analogy for society or for the ordering of the state goes back to the days of Newton himself. One of his disciplines, Jean-Théophile Desaguliers, author of a standard Newtonian textbook, embodied his hopes in a poem,[237] *The Newtonian System of the World, the Best Model of Government*. No political theorist, no practical politician or political leader, and no natural or social scientist ever made use of this curious presentation. Here then is an example of a useless analogy.

There is another early example of useless or inappropriate analogy that is similarly associated with Newton. It is an attempt by a contemporary of Newton's, the Scots mathematician John Craig, to replicate Newtonian science in human affairs. Craig's *Theologiae Christianae Principia Mathematica* (1699) is a direct emulation of Newton's *Philosophiae Naturalis Principia Mathematica*.[238] Craig's aim was to devise a Newtonian law in a social context in the realm of reliability of testimony. The subject he explored was the degree of credence that may be assigned to the testimony of successive witnesses, a topic of major significance in the context of reported miracles. Craig came up with an ingenious Newtonian answer: the reliability of such testimony varies inversely as the square of the time from that testimony to the present, just as the Newtonian gravitational force decreases as the square of the distance. This law is plainly another example of inappropriate analogy.[239]

Despite the hopes of many social scientists, Newton's physics – i.e., the physics expounded by Newton in the *Principia* – has never provided a useful analogy for economics, political science, or sociology. Although post-Newtonian rational mechanics (with non-Newtonian additions by d'Alembert, Euler, Lagrange, Laplace, and Hamilton) proved useful for economics, especially when combined with energy physics, Newtonian rational mechanics by itself was not sufficient to provide a useful model for the social sciences. The reason, I believe, is that the Newtonian system is built on a set of abstractions and conditions that are not realizable in the world of experience. Even the Newtonian system of the world is an abstract concept to the extent to which it cannot be embodied in a mechanical model or picture, in the sense that is possible for the Cartesian system of vortices or even the complex machinery of the Ptolemaic world of epicycles or the Aristotelian universe of nesting spheres. In fact, it was on account of this feature that some of Newton's contemporaries rejected the celestial physics of the *Principia*, criticizing Newton specifically for having deserted the "mechanical philosophy." In any case, the record of history shows that Newton's physics, despite centuries of hope and effort, has not yielded an analogy appropriate for the social sciences.

Social Darwinism provides another significant illustration of an inappropriate analogy.[240] Whereas evolution in general continues to be useful to the social sciences, social Darwinism has left no permanent scientific legacy. Any strict comparison and contrast of the factors operative in Darwinian biological evolution and those determining success in the struggle to succeed in our modern capitalist society will show at once that Darwinian biological evolution provides an inappropriate analogy for such individual social behavior. In analyzing this example, however, great care must be exercised lest the failure of social Darwinism be seen as a simple example of erroneous science. This would be the case only if social Darwinism had been the result of an application to human society of misunderstood principles of Darwin's theory of evolution. But in social Darwinism it was not Darwinian science that was being applied so much as Spencerian principles.[241] Since a major premise of Spencerian evolution is scientifically incorrect, social

Darwinism does exemplify erroneous science. Yet social Darwinism is not in error for an incorrect interpretation of Darwinian science but rather for adoption of Lamarckian principles of heredity, for rejection of Darwinian evolutionary biology in favor of the biology of Spencerian sociology. This case differs from Carey's error in stating the law of gravity because in Spencer's day, at least until after the discoveries of August Weismann, there was no real proof that acquired characters could not be inherited. Scientifically literate contemporaries of Carey knew the correct law of gravity, while Spencer and his contemporaries could still believe in what later would prove to be incorrect science.

Spencer's evolutionary ideas were scientifically incorrect because he accepted the crudest kind of Lamarckism, believing that a consequence must be that individuals can affect, and even direct, the path of their own evolutionary development. Spencer cannot legitimately be faulted for holding to this belief during most of his career, although he can be criticized for his later quixotic efforts to deny Weismann's research on allegedly scientific grounds, as did his American discipline Lester F. Ward. Furthermore, there are ample grounds for questioning whether there can be any permanent value to Spencerian social theory since it was constructed on a strict Lamarckian basis. Spencer himself admitted his incapability of "separating changes in a group's learning repertory from hereditary modifications."[242]

A somewhat related example of the social application of a scientifically inappropriate analogy, proposed by Stephen Gould, also involves Lamarckian evolution.[243] After examining some aspects of changing technologies, Gould concluded that human culture "has introduced a new style of change to our planet." The reason is that "whatever we learn and improve in our lives, we pass to our offspring as machines and written instructions." Since each generation "can add, improve and pass on," there is a "progressive character to our artifacts," and thus the development of culture may be said to be Lamarckian rather than Darwinian. But "whatever we do by dint of struggle to improve our minds," Gould continues, "confers no genetic advantage upon our offspring," who "must learn these skills from scratch using the tools of cultural transmission."[244] Gould concludes that the "fundamental difference between Lamarckian and Darwinian styles of change" may serve to "explain" why "cultural transformation," unlike biological evolution, is "rapid and linear."[245]

Gould, of course, is not guilty of an error in science. A noted paleontologist and evolutionist, he is explicitly aware that the primary feature of Lamarckian evolution to which he refers in a social context – the inheritance of acquired characters – is not an acceptable principle of natural science. And we have seen him declare that we confer "no genetic advantages upon our offspring" by improving our minds. Hence, he must be arguing that a principle of incorrect natural science may be used to construct a useful principle of social science. But is this Lamarckian analogy really useful?

Gould does not develop any further consequences from his Lamarckian suggestion. He does not explore any alterations that might need to be made in current conceptions of social organization or social change, nor does he even suggest a revision of past and present conceptions of the growth of technology and invention. It was apparently Karl Marx who first suggested that the history of technology should be conceived in a Darwinian mode, but Gould does not mention the fact, nor does he explore in what sense Marx may have used either a Lamarckian or a Darwinian model. Others – most recently George Bassalla – have seen the history of technology as exhibiting a strictly Darwinian framework of evolution, in which the non-Lamarckian principle of natural selection is of primary importance. Their work is also ignored by Gould, who merely suggests a possibly Lamarckian thesis without reference to other versions of evolutionary technology. Hence we may legitimately wonder whether this example is introduced primarily as a metaphor in order to express a point of view about society and technology. In any event, since the analogy is not developed and has not proved to be of use in social analysis, we must as of now assign it to the category of the inappropriate.

Some other analogies in the social sciences, even though based on current and correct natural science, may also prove to be useless or inappropriate and even misleading. They may in the end produce confusion and obfuscation rather than illumination. This aspect of analogy was a central issue in a fairly recent intellectual exchange in economics and will further serve to illustrate a fundamental distinction between analogy and metaphor.[246]

In 1950 Armen A. Alchian published an article on "Uncertainty, Evolution and Economic Theory"[247] which called forth a response by Edith Penrose on the general subject of the use of biological analogies in economics. Penrose admitted, at the outset, that economics "has always drawn heavily on the natural sciences for analogies designed to help in the understanding of economic phenomena."[248] She was not concerned with analogies in general but rather with what she saw as a deleterious effect of using "sweeping analogies" in economics: their tendency to frame "the problems they are designed to illuminate" in so special a way that "significant matters are inadvertently obscured." Concentrating on "theories of the firm," she considered three biological analogies used by economists: the life cycle, natural selection (or viability), and homeostasis.

In the course of her critique, Penrose makes an important distinction between two uses of analogies in economics, a distinction which is similar to the typology which I have been presenting in this chapter. One use is to advance our understanding by referring a not fully understood economic phenomenon to an analogous one in some other science which is presumably better understood. The other, which she calls a "purely metaphorical analogy," uses such resemblances "to add a picturesque note to an otherwise dull analysis" and to help the reader in following a difficult argument or in dealing with a strange concept or principle.[249]

Penrose acknowledges that Alchian's argument is not a crude evolutionism, characterized by value judgments such as beset the social Darwinism of the nineteenth century, but is rather "very modern in its emphasis on uncertainty and statistical probabilities." Among the conclusions on which she focuses her criticism are that "successful innovations – regarded by analogy as 'mutations' – are transmitted by imitation to other firms" and that the "economic counterparts of genetic heredity, mutations, and natural selection are imitation, innovation, and positive profits." She sums up the alleged superiority of the evolutionary analogy "in the claim that it is valid even if men do not know what they are doing." That is, "no matter what men's motives are, the outcome is determined not by the individual participants, but by an environment beyond their control." Thus "natural selection is substituted for purposive profit-maximizing behavior just as in biology natural selection replaced the concept of special creation of species."[250]

Penrose makes an excellent case that on every level of homology (although she does not use this term) there is an incompatibility between biology and economics. For example, she shows that humans differ from other animals in their ability to alter the environment and to become, to some degree, independent of it. Furthermore, she detects a serious error in treating "innovations" as homologues (she writes of "analogues") of "biological mutations," since the latter involve an alteration of the "substance of the hereditary constitution," while innovations rather tend to be "direct attempts by firms to alter their environment." Her conclusion is that the biological analogy has hindered rather than advanced Alchian's stated purpose of exploring "the precise role and nature of purposive behavior in the presence of uncertainty and incomplete information." In effect, Penrose finds that the biological analogy fails on two grounds: it is based on a mismatched homology, and it tends to confuse or obscure rather than to clarify the problem at hand, the economics of the firm. Hence, like the Lamarckian analogy, the analogy of natural selection with respect to economics is inappropriate.

In his reply to the critique, Alchian asserted that his theory of the firm "stands independently of the biological analogy," that "every reference to the biological analogy" was "merely expository" and "designed to clarify the ideas in the theory."[251] In her rejoinder, Penrose reasserted her position that, even so, "the biological analogy places the whole problem in a misleading frame of reference."[252] Wholly apart from the merits of one or the other position with respect to the theory of the firm, Penrose insists that the introduction of the analogy of natural selection hinders rather than furthers understanding. This negative effect of an analogy, even though the analogy is based on correct science, is similar in its net result to that of other varieties of inappropriate or useless analogies: it does not help and may even hinder our understanding.

The problems of specific homologies versus general analogies appears prominently in the revised edition of a book on *Social Change* by the sociologist W. F. Ogburn, of which a major stated aim was to "compare the rate of biological change with the rate of cultural change." In the revised version, published in 1950, Ogburn recalled that the first edition appeared in 1922, at a time when there had been a notable decline in the belief that "the theory of social evolution would explain the origin and development of civilization as the theory of biological evolution had explained the origin and development of man." Darwin, Ogburn noted, "had reduced the evolution of species to three causal factors: variation, natural selection, heredity." Evolutionist theories of society had failed, in Ogburn's opinion, because "many investigators were too slavish in copying the biological account in terms of selection, adaptation, survival of the fittest, variation, survival, recapitulation, and successive stages of development."[253] That is, these theories failed because they adopted a literal homology rather than making use of general analogies or metaphor.

1.11. CONCLUSION

The emulation of the natural sciences by a social science carries with it a validation of the methods used and a legitimation of the enterprise in question. Claude Ménard[254] has expressed this beautifully by referring to the "polemical function of an analogy," explaining that analogy "aims at persuasion, looking to a recognized science for a prestigious answer, for the glamour and security of an argument endorsed by the learned and the revered." An example is the authority carried by a report in the social sciences that has the same formal appearance as one produced in chemistry or physics. In the end, however, the worth of the result will not be gauged by its resemblance to, or even direct kinship with, one or another of the natural sciences so much as by the degree to which it serves its own discipline or by its applicability to the solution of some practical problem.

An allied point is that the use of numerical data, accepted statistical techniques, graphs, and other mathematical tools, including computer modeling, not only makes a social science look like physics but also produces results that are quantitative and testable and hence easily susceptible of application. This is, of course, one of the main reasons why physics *is* an "exact" science and why its results tend to have applications with unambiguous results.

Such considerations are notably illustrated by the Coleman Report, submitted to President Johnson and the Congress in 1966.[255] This appears to have been the first report in the social sciences to originate in a specific mandate from the Congress, embodied in Section 402 of the Civil Rights Act of 1964:

> The Commissioner [of Education] shall conduct a survey and make a report to the President and the Congress, within two years of the enactment of this title, concerning the lack of availability of equal educational opportunities for individuals by reason of race, color, religion, or national origin in public educational institutions at all levels in the United States, its territories and possessions, and the District of Columbia.[256]

Although the actual purpose of the survey was never made explicit, it is obvious in retrospect that one of the questions for which the Congress wanted a documented answer was the relative success of students in integrated and segregated schools. It was plain from the outset that whatever the findings of the survey would be, the whole subject was so controversial that the report would have to be based on the most objective kinds of data possible. Not only did the nature of the inquiry demand that the data be quantitative, but there was the obvious requirement that the collection of data be as free of prejudice as possible and that the statistical analyses be free of any fault in technique. In short, the standards to be adopted were much the same as those that would be used in an investigation in physics or any other of the "exact" sciences.

Of course there were aspects of this study that distinguished it from investigations in the physical sciences. For example, the data collected and used for the Coleman Report were much like census data and therefore less certain than numbers in physics.[257] Again, the choice of factors that were enumerated was not quite so value-free as might have been the case for physics.[258] Furthermore, the Coleman Report had to convince Congress and its constituents of the validity of one of the principal "pathbreaking" findings, that an analysis of "the relation of variation in school facilities to variation in levels of academic achievement" showed that there was "so little relation" that, to all intents and purposes, there was none.[259] The implication was that an increase in financial support of and by itself would not necessarily produce better secondary education. This finding constituted a "powerful critique" of one of the most "unquestioned basic assumptions" or "socially received beliefs" of American education. In support of such consequences, the results of the investigation had to be stated unambiguously in the numerical language of quantitative science.

Ever since the Scientific Revolution, a high value has been set on giving social science the solid foundation of the natural sciences. This goal has traditionally had two very different aspects. One, the subject of this chapter, has been of a limited kind: to make use of the concepts, principles, methods, and techniques of some one of the physical or biological sciences. The other has been greater than merely constructing social theories by introducing analogues or homologues of a particular natural science at a particular time. Adopting the metaphor of the natural sciences traditionally has meant taking on certain features of what was known as the scientific method – supposedly characterized by healthy skepticism, reliance on experiment and critical observation, avoidance of pure speculation, and in particular a specific ladder of steps that would lead (usually by induction) from "facts" to "theory," to a knowledge of the eternal "truths" of nature. This second goal, which might from one point of view seem a more obviously useful aim, has actually become increasingly problematic. Twentieth-century philosophers and historians of science, aided by scientists themselves, have dispelled any belief in "the" scientific method. The extreme position was probably stated by P. W. Bridgman when he declared that insofar as there is any "method" in science it is "doing one's damnedest with one's mind, no holds barred." Accordingly, although many social scientists still aspire to have their subject be "like" the "sciences," the quality of likeness no longer features a specific "scientific method."

Moreover, it is widely recognized today that continuous change (usually characterized as "advance") is a principal feature of the natural sciences. The result is that the particular aspects of any natural science being emulated by social sciences will, often without warning, undergo a radical transformation. Accordingly, the present value or usefulness of principles of social science – just as is the case for principles of social and political practice – can not be reckoned primarily by an evaluative contrast between the present state of some part of physical or biological science and the anterior state current when those principles were being formulated. It is admittedly of general interest and major historical concern to discern whether the economic thought of Adam Smith or of François de Quesnay was in part based on Newtonian or on Cartesian principles of science, but the validity and usefulness of their concepts is not dependent on the present validity of the natural science that originally inspired them. Similarly, the worth of Darwinian evolutionary ideas in sociology or in anthropology has been judged primarily in relation to their use for those social sciences and has not exactly parallelled the ups and downs of scientific consensus on the Darwinian concept of natural selection.

The feature of dramatic change is seen in stark relief in what was long held to be the most paradigmatic of the exact sciences, Newtonian rational mechanics. In the last two centuries, this subject has been altered by the introduction of new principles, such as those associated with d'Alembert, Lagrange, Laplace, and Hamilton, and by the addition of considerations of energy and variational principles; there has been a dramatic and even more radical reconstruction of the whole subject as a result of Einsteinian relativity. And it is much the same in the shift from classical to quantum physics or from the older natural history to molecular biology.

For the historian, the study of interactions between the natural sciences and the social sciences takes on the added dimension of interest because of the feature of change. Historians cannot fail to be impressed when finding that the validity of concepts, principles, laws, and theories in the social sciences transcends the corresponding present validity of the counterparts in the natural sciences that served as the original sources of inspiration or of generation of ideas. This is merely another way of saying that the social sciences have developed an autonomy and do not merely have the status of being instances of applied physical or biological science. This conclusion underlines the importance of the study of history in any study of the methodology, and even of the legitimacy, of the social sciences.

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NOTES

* All of the examples introduced in this introductory section are discussed in full, with bibliographic documentation, in the succeeding sections.

[1] On Cournot's important contributions to mathematical economics, see Claude Ménard: *La formation d'une rationalité économique: A.A. Cournot* (Paris: Flammarion, 1978); also Joseph Schumpeter: *History of Economic Analysis* (New York: Oxford University Press, 1954).

[2] See Note on Natural Science & Social Science on pp. xxv–xxxvi.

[3] On the history of the concept of behavioral sciences, see Bernard Berelson on "Behavioral Sciences" in *International Encyclopedia of the Social Sciences*, ed. David L. Sills, vol. 2 (New York: The Macmillan Company & The Free Press, 1968), pp. 41–45; also Herbert J. Spiro: "Critique of Behavioralism in Political Science," pp. 314–327 of Klaus von Beyme (ed.): *Theory and Politics, Theorie und Politik, Festschrift zum 70. Geburtstag für Carl Joachim Friedrich* (The Hague: Martinus Nijhoff, 1971).

[4] An admirable discussion of the differences in these three usages is given in John Theodore Merz: *A History of European Thought in the Nineteenth Century* (Edinburgh/London: W. Blackwood and Sons, 1903–1914; reprint, New York: Dover Publications, 1965; reprint, Gloucester: Peter Smith, 1976), vol. 1, chs. 1, 2, 3.

[5] See Marie Boas Hall: *All Scientists Now: the Royal Society in the Nineteenth Century* (Cambridge/New York: Cambridge University Press, 1984); also Dorothy Stimpson: *Scientists and Amateurs: A History of the Royal Society* (New York: Henry Schuman, 1948). The Royal Society, however, originally had a large proportion of non-scientists as Fellows, including poets (e.g., John Dryden), doctors, and peers of the realm. After the reorganization of 1847 the non-scientific categories of membership were eliminated, although exceptions are still made (e.g. Prince Philip and the financier-philanthropist Isaac Wolfson).

[6] See n. 21 infra; also Chapter 12 infra.

[7] See Roger Hahn: *The Anatomy of a Scientific Institution: The Paris Academy of Sciences* (Berkeley: University of California Press, 1971).

[8] At first there were four classes: "Physica" (including chemistry, medicine, and other natural sciences), "Mathematica" (including astronomy and mechanics), German philosophy, and literature (especially oriental literature). Later these classes were regrouped into

two major divisions: the natural sciences and mathematics in one and "philosophical and historical" domains in the other. See Erik Amburger (ed.): *Die Mitglieder der Deutschen Akademie der Wissenschaften zu Berlin, 1700–1950* (Berlin: Akademie-Verlag, 1960); Kurt-Reinhard Biermann & Gerhard Dunken (eds.): *Deutsche Akademie der Wissenschaften zu Berlin: Biographischer Index der Mitglieder* (Berlin: Akademie-Verlag, 1960). On the history and vicissitudes of the German Academy, see Werner Hartkopf & Gerhard Dunken: *Von der Brandenburgischen Sozietät der Wissenschaften zur Deutschen Akademie der Wissenschaften zu Berlin* (Berlin: Akademie-Verlag, 1967); the standard history is Adolph Harnack: *Geschichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin*, 3 vols. (Berlin: Reichsdruckerei, 1990).

[9] See note 2 supra.

[10] See Samuel Johnson: *A Dictionary of the English Language*, 2 vols. (London: printed by W. Strahan for J. and P. Knapton, T. and T. Longman, C. Hitch and L. Hawes, A. Millar and R. and J. Dodsley, 1755; photo-reprint, New York: Arno Press, 1979).

[11] *The Works of Lord Macaulay*, ed. Lady Trevelyan, vol. 5 (London: Longmans, Green, and Co., 1871), p. 677. In 1829 Macaulay wrote (ibid, p. 270) that the "noble Science of Politics" was "of all sciences . . . the most important to the welfare of the nations," the science which "most tends to expand and invigorate the mind." Additionally, he declared, the "Science of Politics" is notable among "all sciences" because it "draws nutriment and ornament from every part of philosophy and literature, and dispenses in return nutriment and ornament to all." See also Stefan Collini, Donald Winch, & John Burrow: *That Noble Science of Politics* (Cambridge: Cambridge University Press, 1983), passim, but esp. pp. 102–103, 120. The negative version of Macaulay's statement occurs in the oft-quoted remark made by Bismarck in the Prussian Chamber on 18 December 1863, "Die Politik ist keine exakte Wissenschaft," that is, "Politics is not an exact science." Early in the eighteenth century, in *Gulliver's Travels* (1726), Jonathan Swift regretted that the ignorant Brobdignagians had not as yet "reduced Politicks into a *Science*."

[12] " A Letter to a Noble Lord," in Edmund Burke: *The Works* (London: John C. Nimmo, 1887; reprint, Hildesheim/New York: Georg Olms Verlag, 1975), vol. 5, p. 215; David Hume: *A Treatise of Human Nature*, ed. L.A. Selby-Bigge (Oxford: Clarendon Press, 1896 and reprints), pp. ix and xix–xx. In *An Inquiry Concerning the Human Understanding*, ed. L.A. Selby-Bigge (Oxford: Clarendon Press, 1894), pp. 83–84, Hume wrote of historical "records" as "so many collections of experiments, by which the politician or moral philosopher fixes the principles of his science, in the same manner as the physician or natural philosopher becomes acquainted with the nature of plants, minerals, and other external objects, by the experiments which he forms concerning them."

On Hume and a science of politics, the studies of Duncan Forbes are of primary importance, notably his introduction to the reprint of Hume's *History of Great Britain* (Harmondsworth: Penguin, 1970); "Sceptical Whiggism, Commerce and Liberty," pp. 179–201 of A.S. Skinner & T. Wilson (eds.): *Essays on Adam Smith* (Oxford: Oxford University Press, 1976); "Hume's Science of Politics," pp. 39–50 of G.P. Morice (ed.): *David Hume, Bicentenary Papers* (Edinburgh: Edinburgh University Press, 1977). See also James E. Force & Richard H. Popkin: *Essays on the Context, Nature, and Influence of Isaac Newton's Theology* (Dordrecht/Boston/London: Kluwer Academic Publishers, 1990), ch. 10, "Hume's Interest in Newton and Science" (by J.E. Force).

[13] John Harris, *Lexicon Technicum* (London: printed for Dan. Brown, Tim. Goodwin,

John Walthoe, . . . , Benj. Tooke, Dan. Midwinter, Tho. Leigh, and Francis Coggan, 1704; reprint, New York/London: Johnson Reprint Corporation, 1966 – The Sources of Science, no. 28) defines "Science" as "Knowledge founded upon, or acquir'd by clear, certain and self-evident Principles." Harris says that "Natural Philosophy" is "the same with what is usually call'd *Physicks, viz.* That Science which contemplates the powers of Nature, the properties of Natural Bodies, and their mutual action one upon another."

[14] James S. Coleman: *Foundations of Social Theory* (Cambridge: The Belknap Press of Harvard University Press, 1990), p. xv. On this general topic, see Robert K. Merton: *Social Theory and Social Structure* (Enlarged edition, New York: The Free Press, 1968), ch. 1, "On the History and Systematics of Sociological Theory."

[15] Robert K. Merton: "The Mosaic of the Behavioral Sciences," pp. 247–272 of Bernard Berelson (ed.): *The Behavioral Sciences Today* (New York/London: Basic Books, 1963), esp. p. 256.

[16] A. Hunter Dupree: *Science in the Federal Government: A History of Policies and Activities* (Cambridge: Belknap Press of Harvard University Press, 1957; revised reprint, Baltimore: Johns Hopkins University Press, 1986).

[17] Reported in Henry W. Riecken: "The National Science Foundation and the Social Sciences," *Social Science Research Council Items*, Sept. 1983, **37**(2/3): 39–42, esp. p. 40*a*.

[18] For example, the association of "sociology" with "socialism" is discussed in Albion W. Small & George E. Vincent: *An Introduction to the Study of Society* (New York/Cincinnati/Chicago: American Book Company, 1894), pp. 40–41, where it is stated that "Systematic Socialism has both directly and indirectly promoted the development of Sociology."

[19] Riecken (n. 17 supra), p. 40*b*.

[20] See Chapter 12 infra.

[21] The Ninth Annual Report of the National Science Foundation announced that "during the fiscal year 1959 [in December 1958], the Foundation established an Office of Social Sciences to support research and related activities in the social science disciplines." The Eleventh Annual Report announced that "The Office of Social Sciences was reconstituted as the Division of Social Sciences during fiscal year 1961 [the year ending on 30 June 1961]." The new Division replaced "the previous Social Science Research Program" and was said to represent "a further step in the development of Foundation activities in the area." The social sciences did not maintain this independent importance, however, and there is still some debate on how best to fit the social sciences into the structure of the National Science Foundation.

A note in *Science* (16 Aug. 1991, **253**: 727) on "a proposal to give the social sciences more clout within the agency" summarized the findings of a draft report by a "task force composed of 20 outside social and behavioral scientists and biologists" who recommended the removal of the social and behavioral sciences from their position as part of the Directorate for Biological, Behavioral and Social Sciences. Although most "social scientists" were in favor of this proposal, which would "give them an advocate at the highest level of the agency and win them more funding and respect," some social scientists – it was noted – "don't want to leave the fold." On 23 Oct. 1991, *The Chronicle of Higher Education* (pp. A-23, A-25) reported that the reorganization had occurred, including the establishment of a new Directorate for Social, Behavioral and Economic Sciences. NSF Director Walter E. Massey expressed high hopes that the creation of a "separate new office" would "lead to an increase in funds for those sciences." Although many "social scientists said the change would lead to more recognition and higher budgets," support for the change "was not unanimous" (according to the *Chronicle*'s report) even among the social scientists concerned.

22 The work of scientists and social scientists mentioned briefly in various sections of this chapter is discussed at greater length and with bibliographical documentation in subsequent chapters or in other sections of this chapter.

23 Wilhelm Ostwald: *Energetische Grundlagen der Kulturwissenschaften* (Leipzig: Verlag von Dr. Werner Kilinkhardt, 1909). On Ostwald's "Kulturwissenschaften" see Philip Mirowski: *More Heat than Light: Economics as Social Physics, Physics as Nature's Economics* (Cambridge/New York: Cambridge University Press, 1989), pp. 454–57, 132–133, 268.

There were many scientists and social scientists who saw in the new "energetics" a basis for a reconstitution of economics, sociology, history, and so on. A notable example was Henry Adams, who attempted to use J. Willard Gibbs's memoir on the "Equilibrium of Heterogeneous Substances" as the basis of a study on "The Rule of Phase Applied to History." This essay is reprinted along with Adam's "A Letter to American Teachers of History" in Brooks Adams (ed.): *The Degradation of the Democratic Dogma* (New York: The Macmillan Company, 1920).

24 See Chapter 2 infra.

25 See John Brewer: *The Sinews of Power: War, Money and the English State, 1688–1783* (Cambridge: Harvard University Press, 1988), ch. 8, "The Politics of Information, Public Knowledge and Private Interest"; Keith Thomas: "Numeracy in Early Modern England," *Transactions of the Royal Historical Society*, 1987, **37**: 103–132.

26 See Chapter 4, §3, infra.

27 See Jacques Roger: *Buffon* (Paris: Fayard, 1989), pp. 234, 296.

28 Stephen M. Stigler: *The History of Statistics: The Measurement of Uncertainty before 1900* (Cambridge: The Belknap Press of Harvard University Press, 1986), ch. 3, "Inverse Probability." See further, Helen M. Walker: *Studies in the History of Statistical Method* (Baltimore: The Williams & Wilkins Company, 1929; reprint, New York: Arno Press, 1975), pp. 31–38; Hyman Alterman: *Counting People: The Census in History* (New York: Harcourt, Brace & World, 1969).

29 Keith M. Baker: *Condorcet, from Natural Philosophy to Social Mathematics* (Chicago: University of Chicago Press, 1975), ch. 4.

30 See Chapter 3 infra. In addition to Stigler's *History of Statistics* (n. 28 supra), ch. 5, see Theodore M. Porter: *The Rise of Statistical Thinking, 1820–1900* (Princeton: Princeton University Press, 1986), esp. pp. 2, 4; Ian Hacking: *The Taming of Chance* (Cambridge/New York: Cambridge University Press, 1990), chs. 13, 14, 19, 21; Frank H. Hankins: *Adolphe Quetelet as Statistician* (New York: Columbia University Press, 1908 – Studies in History, Economics and Public Law; reprint, New York: AMS Press, 1968).

31 Baker (n. 29 supra), p. 202.

32 Mirowski (n. 23 supra), ch. 5. Mirowski's thesis has not produced universal acceptance by economists. Not only is it considered extreme, but it is faulted because it does not apply to all founders of neoclassical economics, for example, Karl Menger, and even Léon Walras (see n. 33 infra and §1.5 infra).

33 See further, §1.6 infra. Walras, we shall have occasion to note, argued for the

similarity of economics and rational mechanics but (see n. 143 infra) did so only after he had produced his system of economics. That is, he did not create his economics by attempting to imitate rational mechanics.

[34] See §1.5 infra.

[35] William Breit & Roger W. Spencer (eds.): *Lives of the Laureates: Seven Nobel Economists* (Cambridge: MIT Press, 1986), p. 74. This topic is developed further in §1.6 infra.

[36] In addition to the works cited in nn. 28, 30 supra, see Gerd Gigerenzer, Zeno Swijtink, Theodore Porter, Lorraine Daston, John Beatty, and Lorenz Krüger: *The Empire of Chance: How Probability Changed Sciences and Everyday Life* (Cambridge/New York: Cambridge University Press, 1989); William Coleman: *Death is a Social Disease: Public Health and Political Economy in Early Industrial France* (Madison: University of Wisconsin Press, 1982).

[37] David Brewster: *Letters on Natural Magic* (New York: Harper & Brothers, 1843), p. 181. In *The Glaciers of the Alps* (Boston: Ticknor & Fields, 1861), p. 285, John Tyndall wrote that "the analogy between a river and glacier moving through a sinuous valley is therefore complete."

[38] Richard Owen defined these two terms as follows: analogue – "A part or organ in one animal which has the same function as another part or organ in a different animal"; homologue – "The same organ in different animals under every variety of form and function." Richard Owen: *On the Archetypes and Homologies of the Vertebrate Skeleton* (London: Richard & John E. Taylor, 1848), p. 7. Despite Owen's phrasing here, the terms "similarity of form" or "sameness of structure" may be used to represent the kind of likeness exemplified by "the same organ." See further n. 40 infra; also Mayr (n. 40 infra), p. 464.

[39] In Darwinian evolution, analogy is the result of parallel adaptation, the way in which different organisms in separate but parallel evolutionary stages have developed, independently of one another, different ways of "adapting themselves to the same external circumstances" or needs. An example is given by an organ of vision, in which a lens concentrates light on special sensitive tissue. Konrad Lorenz has noted that this "invention" had been made independently by animals of four different phyla, in two of which (the vertebrates and the cephalopods) this kind of "eye" has "evolved into the true, image-projecting camera through which we ourselves are able to see the world." See Konrad Z. Lorenz: "Analogy as a Source of Knowledge," *Science*, 1974, **185**: 229–234.

[40] See Ernst Mayr: *The Growth of Biological Thought: Diversity, Evolution, and Inheritance* (Cambridge: The Belknap Press of Harvard University Press, 1982), p. 45, where it is noted that the "term 'homologous' existed already prior to 1859, but it acquired its currently accepted meaning only when Darwin established the theory of common descent. Under this theory the biologically most meaningful definition of 'homologous' is: 'A feature in two or more taxa is homologous when it is derived from the same (or a corresponding) feature of their common ancestor.'"

[41] "Homology" appears with special meanings in several sciences. In addition to the general evolutionary or biological sense, there is the chemical usage (referring to a family of organic compounds in which each member is distinguished from the next in the sequence by some constant factor, notably, a CH_2 group), the mathematical usage (a

topological classification), and a special usage in genetics (to indicate the same linear sequence of genes in two or more chromosomes).

[42] It should be noted that the terms "analogue" and "homologue" are not being used in the present context in the strict sense of evolutionary biology, since in the analysis of the interactions of the social and the natural sciences there is no consideration of "common descent." Furthermore, because "analogy" is often used to designate various types of correspondence, it is sometimes necessary, especially in quoting or paraphrasing the work of others, to employ this term to indicate likeness in more general senses than those specified above.

[43] Henry C. Carey: *Principles of Social Science* (Philadelphia: J. B. Lippincott & Co., 1858), vol. 1, pp. 42–43.

[44] Carey's exact words are: "Gravitation is here, as everywhere else in the material world, in the direct ratio of the mass, and in the inverse one of the distance." In vol. 3, ch. 55, p. 644, Carey recapitulates his physics and social science. He begins by stating "simple laws which govern matter in all its forms, and which are common to physical and social science." The first of these reads: "All particles of matter gravitate towards each other – the attraction being in the direct ratio of the mass, and the inverse one of the distance." Incidentally, it may be observed that Carey has also misunderstood the Newtonian explanation of orbital or curved motion, under the actions of a centripetal force, such as a planet moving under the action of the sun's gravity plus its own component of inertia. Carey says: "All matter is subjected to the action of the centripetal and the centrifugal forces – the one tending to the production of local centres of action, the other to the destruction of such centres, and the production of a great central mass, obedient to but a single law." We may take note that Carey also introduced ratios other than direct and inverse proportion. Thus, in vol. 1, p. 389, he wrote: "The motion of society, and the power of man, tend to increase in a geometrical ratio. . . ."

[45] Although "fallacy" is often used in a narrow technical sense to denote a flaw (or type of flaw) that "vitiates a syllogism," a primary meaning in every dictionary I have consulted (*OED*, *OED*-suppl., *OED* – 2nd ed.; *Concise Oxford Dictionary* – 6th ed.; *Webster's New International* – 2d & 3d eds.) is a misleading argument, or a delusion or error, or some unsoundness or delusiveness or disappointing character of an argument or belief. *The American Heritage Dictionary* gives as the first meaning: "An idea or opinion founded on mistaken logic or perception; a false notion"; other meanings include "the quality of being deceptive" and "incorrectness of reasoning or belief." The only example given is a "romantic fallacy, that Shakespeare was superhuman." This example displays features in common with two frequently encountered uses of "fallacy" today: John Ruskin's notion of the "pathetic fallacy" (in which inanimate objects are supposed to have human emotions) and W.K. Wimsatt and Monroe Beardsley's "intentional fallacy" (overstressing the author's intentions in assessing a literary work). These usages are somewhat similar to Alfred North Whitehead's "fallacy of misplaced concreteness" as presented in *Science and the Modern World* (New York: The Macmillan Company, 1931), ch. 4, pp. 82, 85.

[46] Newton's concept of mass has two separate aspects: one (inertial mass in post-Einstein terminology) is a measure of body's resistance to being accelerated or being made to undergo a change in "state," while the other (gravitational mass) is a measure of a body's response to a given gravitational field (i.e., the weight). For details see my *The Newtonian*

Revolution: with Illustrations of the Transformation of Scientific Ideas (Cambridge/London/New York: Cambridge University Press, 1980).

Newton recognized that in ordinary (i.e., non-relativistic) rational mechanics there is no logical reason why these two concepts or measures of mass should be equivalent. Accordingly, he instituted a series of experiments to show that one is always proportional to the other, that at any given location mass is proportional to weight. These experiments are described in Book 3, prop. 6, of the *Principia*, which reports how he experimented with "gold, silver, lead, glass, sand, common salt, wood, water, and wheat" and could have easily detected a variation of as little as one part in a thousand. Newton, of course, did not use such terms as "gravitational mass" or "inertial mass" but rather proved that for all such materials the ratio of the "weight" to "quantity of matter" (or mass) was the same.

[47] William Jaffé: "Léon Walras's Role in the 'Marginal Revolution' of the Late 1870s," pp. 115–119 of R.D. Collison Black, A.W. Coates, and Craufurd D.W. Goodwin (eds.): *The Marginal Revolution in Economics: Interpretation and Evaluation* (Durham: Duke University Press, 1973).

[48] For a later attempt by Walras to argue that his economics is analogous to Newtonian rational mechanics, see Philip Mirowski & Pamela Cook: "Walras' 'Economics and Mechanics': Translation, Commentary, Context," pp. 189–224 of Warren J. Samuels (ed.): *Economics as Discourse* (Boston/Dordrecht/London: Kluwer, 1990).

[49] Berkeley, for example, produced a very significant critique of the foundations of Newton's theory of fluxions, that is, Newton's version of the calculus. His *Siris* was an attempt "to assimilate Newtonian concepts to the more complex phenomena of chemistry and animal physiology." In his *De motu* he analyzed "Newtonian concepts of gravitational attraction, action and reaction, and motion in general." See Gerd Buchdahl: "Berkeley, George," *Dictionary of Scientific Biography*, vol. 2 (New York: Charles Scribner's Sons, 1970), pp. 16–18.

[50] See §1.9 infra.

[51] For Newton, the motion resulting from an equilibrium of forces can *only* be constant speed along a straight line, not curved motion as along a planetary orbit. Newton's analysis of orbital or curved motion was based on the concept of two independent components. One is an initial component of inertial (or linear) motion, the other a constantly accelerated motion of falling inward toward the center of force. A planet or other orbiting body, of course, does not actually move inward away from the orbit, even though it is constantly falling toward the center; the reason is that the forward motion along the tangent carries that body ahead at such a rate that it continually "falls" away from the tangent to the orbit. Newton said he gave the centrally directed force the name "vis centripeta" in honor of Christiaan Huygens who had made use of the opposite kind of force, "vis centrifuga." For details, see my *Newtonian Revolution* (n. 46 supra). Since orbital motion involves the constant inward (or centrally directed) acceleration of falling, there is no condition of equilibrium.

[52] Berkeley fully understood Newton's explanation. He gave the correct Newtonian reason why the planets do not actually fall inward so as to join together at the center. They "are kept from joining together at the common centre of gravity," he wrote, "by the rectilinear motions the Author of nature hath impressed on each of them." This tangential or linear component, he continued, "concurring with the attractive principle," produces

"their respective orbits round the sun." He concluded that if this linear component of motion should cease, "the general law of gravitation that is now thwarted would show itself by drawing them all into one mass" (George Berkeley: "The Bond of Society," *Works*, ed. A.A. Luce and T.E. Jessop, vol, 7 [London/Edinburgh: Thomas Nelson and Sons, 1955], pp. 226–227).

[53] Ibid., pp. 225–228; cf. George Berkeley: "Moral Attraction," *Works*, ed. Alexander Campbell Fraser, vol. 4 (Oxford: Clarendon Press, 1901), pp. 186–190.

[54] For additional materials concerning Berkeley's Newtonian sociology, see my "Newton and the Social Sciences, with special reference to Economics: The Case of the Missing Paradigm," to appear in Philip Mirowski (ed.): *Markets Read in Tooth and Claw* (Cambridge/New York: Cambridge University Press, 1993 [in press]) – Proceedings of a Symposium at Notre Dame on "Natural Images in Economics," October 1991.

[55] The eminent sociologist Pitirim A. Sorokin translated Berkeley's correct Newtonian physics into a hodgepodge of incorrect pre-Newtonian explanations. Sorokin not only would have Berkeley make use of the misleading notion of a balance of centrifugal and centripetal forces, but continued his travesty by saying that Berkeley concluded that "Society is stable when the centripetal forces are greater than the centrifugal." This is plainly nonsense even in pre-Newtonian physics; if the centripetal forces should be greater than the centrifugal forces, then obviously there would be no stability but an instability, a lack of balance or equilibrium, and a resultant motion inward, as Berkeley clearly stated would be the case under such circumstances. See Pitirim A. Sorokin: *Contemporary Sociological Theories* (New York/London: Harper & Brothers, 1928), p. 11.

[56] See the writings of Duncan Forbes and of James E. Force (n. 12 supra).

[57] David Hume: *A Treatise of Human Nature* (n. 12 supra), pp. 12–13.

[58] If, as Hume believed, human behavior and social action are regulated by social laws, there is implied the possibility of a social science, one in which – as Hume wrote – "consequences almost as general and certain may sometimes be deduced . . . as any which the mathematical sciences afford us." Seeking to establish a kind of psychology of individual action, Hume seems to have envisioned the construction of a new theoretical science that would ultimately find expression in practice. On the certainty of social laws compared to mathematics, see David Hume: "That Politics may be Reduced to a Science," *Essays: Moral, Political, and Literary*, ed. T.H. Green & T.H. Grose (London: Longman, Green and Co., 1882; reprint, Aalen [Germany]: Scientia Verlag, 1964), vol. 1, p. 99.

[59] Cf. *Design for Utopia: Selected Writings of Charles Fourier*, intro. Charles Gide, new foreword by Frank E. Manuel, trans. Julia Franklin (New York: Shocken Books, 1971 [orig. *Selections from the Works of Fourier* (London: Swan Sonnenschein & Co., 1901]), esp. p. 18; *The Utopian Vision of Charles Fourier: Selected Texts on Work, Love, and Passionate Attraction*, trans., ed., intro. Jonathan Beecher and Richard Bienvenu (Boston: Beacon Press, 1971), esp. pp. 1, 8, 10, 81, 84; *Harmonian Man: Selected Writings of Charles Fourier*, ed. Mark Poster, trans. Susan Hanson (Garden City: Doubleday & Company – Anchor Books, 1971). On Fourier, see Nicholas Y. Riasanovsky: *The Teachings of Charles Fourier* (Berkeley/Los Angeles: University of California Press, 1969) and Frank E. Manuel: *The Prophets of Paris* (Cambridge: Harvard University Press, 1962).

[60] It is a fact of record that groups of idealists actually founded Fourierist utopian colonies

along the bizarre lines he suggested and that Fourierism became a considerable political force in several countries.

[61] Emile Durkheim: *The Division of Labor in Society*, trans. George Simpson (New York: The Free Press, 1933; reprint 1964), p. 339. Cf. Durkheim, *De la division du travail social: étude sur l'organisation des sociétés supérieures* (Paris: Félix Alcan, Éditeur, 1893), p. 378; Durkheim, *De la division du travail social*, 5th ed. (Paris: Librairie Félix Alcan, 1926), p. 330. The first and fifth editions are identical at this point.

[62] Ibid., trans., p. 262. In the *Principia* Newton defines a measure of matter which he calls "quantity of matter" (used as a synonym for "body" or "mass") and which he says is proportional to the volume (or "bulk") and density. Durkheim seems to use both volume and mass in the sense of volume; cf., e.g., trans., pp. 262, 266, 268, 339.

[63] Ibid., trans., p. 268. Furthermore (p. 270), the "division of labor is . . . a result of the struggle for existence, but it is a mellowed *dénouement*. Thanks to it, opponents are not obliged to fight to a finish, but can exist one beside the other. Also, in proportion to its development, it furnishes the means of maintenance and survival to a greater number of individuals who, in more homogeneous societies, would be condemned to extinction."

[64] Ibid., trans., pp. 256–282.

[65] Ibid., trans., p. 266. The example was taken, with a direct citation, from Darwin's *Origin of Species*. Darwin, according to Durkheim, found that "in a small area, opened to immigration, and where, consequently, the conflict of individuals must be acute, there is always to be seen a very great diversity in the species inhabiting it. He found turf three feet by four which had been exposed for long years to the same conditions of life nourishing twenty species of plants belonging to eighteen genera and eight classes. This clearly proves how differentiated they are." This was offered in proof of Darwin's observations "that the struggle between two organisms is as active as they are analogous." Since they have "the same needs" and pursue "the same objects," they are rivals. Eventually, as their numbers increase, the resources available no longer suffice for all, and a struggle for survival ensues. But, "if the co-existing individuals are of different species or varieties," they "do not feed in the same manner, and do not lead the same kind of life," and so they "do not disturb each other." What is perhaps most remarkable about Durkheim's argument based on Darwin is the fact that he referred to Darwin at all. It must be kept in mind that at this time, and for many decades afterwards, Darwinian evolution based on natural selection was not regarded with favor by the French scientific establishment.

[66] Durkheim (n. 61 supra, trans.),p. 336; cf. p. 339.

[67] On the uses of organic analogies, see further §1.7 infra. For Theodore Roosevelt, see his *Biological Analogies in History* (New York: Oxford University Press; London: Henry Frowde, 1910); also *Works*, vol. 12 (New York: Charles Scribner's Sons, 1926), pp. 25–60. A. Lawrence Lowell's organismic views of society may be found in numerous works, notably "An Example from the Evidence of History," pp. 119–132 of *Factors Determining Human Behavior* (Cambridge: Harvard University Press, 1937).

[68] Thomas Carlyle: *Sartor Resartus*, introd. H. D. Traill, *The Works of Thomas Carlyle*, 30 vols. (London: Chapman and Hall, 1896–1899; reprint, New York: AMS Press, 1969), vol. 1, p. 172. See Frederick W. Roe: *The Social Philosophy of Carlyle and Ruskin* (New York: Harcourt, Brace & Co., 1921); also David George Hale: *The Body Politic:*

A Political Metaphor in Renaissance English Literature (The Hague/Paris: Mouton, 1971), pp. 134–135.

[69] Thomas Carlyle: *Past and Present* (London: Chapman and Hall, 1843); *Works* (n. 68 supra), vol. 10, p. 137; "Chartism," *Works*, vol. 29, p. 129.

[70] A brief account of Bluntschli's life and career by Carl Brinkmann can be found in *Encyclopaedia of the Social Sciences*, vol. 2 (New York: The Macmillan Co., 1937), p. 606. See also Francis William Coker: *Organismic Theories of the State* (New York: Columbia University; Longmans, Green & Co., Agents; London: P.S. King & Son, 1910 – Studies in History, Economics and Public Law, vol. 38, no. 2, whole n. 101), pp. 104–114. See, further, J.C. Bluntschli: *Denkwürdiges aus meinen Leben*, 3 vols. (Nördlingen: C. H. Beck, 1884); also Friedrich Meili: *J.C. Bluntschli und seine Bedeutung für die moderne Rechtswissenschaft* (Zurich: Drell Füssli, 1908).

[71] Johann Caspar Bluntschli: *Lehre vom modernen Staat*, 6th ed. (Stuttgart: J.G. Cotta, 1885–1886), the first volume of which was translated into English as *Theory of the State* (Oxford: Oxford University Press, 1892); *Psychologische Studien über Staat und Kirche* (Zurich/Frauenfeld: C. Beyel, 1844). Bluntschli was also author of a widely used reference work, *Deutsches Staats-Wörterbuch* (Stuttgart/Leipzig: Expedition des Staats-Wörterbuchs, 1857–1870).

[72] On Rohmer, see Coker (n. 70 supra), pp. 49–60.

[73] Bluntschli's discussion of the sixteen psychological functions of the state was pilloried by Charles E. Merriam: "The Present State of the Study of Politics," *The American Political Science Review*, 1921, **15**: 173–185. Merriam (p. 183) wrote of "Bluntschli's fearfully and wonderfully made 'political psychology,' in which he compared sixteen selected parts of the human body with the same number or organs in the body politic."

[74] *Psychologische Studien über Staat und Kirche* (n. 71 supra), pp. 54, 86–87, cited in translation in Werner Stark: *The Fundamental Forms of Social Thought* (London: Routledge & Kegan Paul, 1962), pp. 61–62.

[75] Ibid.

[76] Ibid.

[77] On Lilienfeld's life and career see §1.8 infra and esp. n. 182 infra.

[78] Paul von Lilienfeld: *La pathologie sociale* (n. 183 infra), p. 59.

[79] Ibid.

[80] Ibid., pp. 59–60.

[81] J. D. Y. Peel: *Herbert Spencer: The Evolution of a Sociologist, (New York: Basic Books, 1971); J.W. Burrow:* Evolution and Society: A Study in Victorian Social Theory (Cambridge: Cambridge University Press, 1970); David Wiltshire: *The Social and Political Thought of Herbert Spencer* (Oxford: Oxford University Press, 1978).

Of a wholly different sort is the analysis of Spencer in Robert J. Richards: *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior* (Chicago/London: The University of Chicago Press, 1987). Richards has made a careful study of Spencer's ideas based on extensive reading and analysis; in particular he has given us a new understanding of Spencer's social views and biological concepts in relation to the main currents of thought in these areas during Spencer's lifetime. For an anti-Spencerian point of view, see Derek Freeman: "The Evolutionary Theories of Charles Darwin and Herbert Spencer," *Current Anthropology*, 1974, **15**: 211–221. See, further, John C. Greene: *Science, Ideology and World View: Essays in the History of Evolutionary Ideas* (Berkeley/Los

Angeles/London: University of California Press, 1981), ch. 4, "Biology and Social Theory in the Nineteenth Century: Auguste Comte and Herbert Spencer"; for a rebuttal, see Ernst Mayr: *Toward a New Philosophy of Biology: Observations of An Evolutionist* (Cambridge/London: The Belknap Press of Harvard University Press, 1988), essay 15, "The Death of Darwin?".

82 Peel (n. 81 supra), p. 178.

83 Ibid.; see Herbert Spencer: *Essays: Scientific, Political, and Speculative*, vol. 1 (New York: D. Appleton & Co., 1883), "The Social Organism," pp. 287–289.

84 See Peel (n. 81 supra), ch. 7 "The Organic Analogy," with comparative examples of Spencer's use of analogies from physics. For the context of Spencer's analogies, see Richards (n. 81 supra).

85 Spencer (n. 83 supra), pp. 277–279, 283–286.

86 Herbert Spencer: *Essays Scientific, Political, and Speculative*, vol. 3 (New York: D. Appleton & Co., 1896), "Specialized Administration," pp. 427–428.

87 René Worms: *Organisme et société* (Paris: V. Giard & W. Brière, 1896), p. 73.

88 Walter Cannon: "Relations of Biological and Social Homeostasis," pp. 305–324 in his *The Wisdom of the Body* (New York: W. W. Norton & Company, 1932; revised in 1939).

89 Ibid., pp. 309–310. The significance of the cell theory as a source of analogues for social theory is discussed in §1.8 infra.

90 Ibid., pp. 312, 314.

91 See Merton (n. 14 supra), ch. 3, pp. 101n, 102–103.

92 Walter Cannon: "The Body Physiologic and the Body Politic," Presidential Address to the American Association for the Advancement of Science, in *Science*, 1941, **93**: 1–10.

93 Ibid.

94 Henry C. Carey: *The Unity of Law* (Philadelphia: Henry Carey Baird, 1872), pp. 116–127; for a derisive critique see Stark (n. 74 supra), pp. 156–160.

95 See the discussion of the Newtonian style at the end of §1.5 infra and in my article on "Newton & the Social Sciences," cited in n. 54 supra.

96 In fact, there are systems of social thought based on models from physics that seem just as ridiculous as those based on biological models, such as Carey's extravagant electrical analogy (n. 94 supra). Another type of extreme model is set forth in Bradford Peck: *The World a Department Store: A Story of Life Under a Cooperative System* (Lewiston [Me.]: B. Peck, c1900).

97 See the valuable discussion of these topics in Claude Ménard: "La machine et le coeur: essai sur les analogies dans le raisonnement économique," in *Analogie et Connaissance*, vol. 2: *De la poésie à la science* (Paris: Maloine éditeur, 1981 – Séminaires Interdisciplinaires du Collège de France), pp. 137–165; also trans. Pamela Cook & Philip Mirowski as "The Machine and the Heart: An Essay on Analogies in Economic Reasoning," *Social Concept*, December 1988, **5** (no. 1): 81–95. Especially since the translation omits the mathematical appendix and the discussion, it is well to consult the original.

98 Stark (n. 74 supra), pp. 73–74.

99 In this connection we may recall once more Whitehead's presentation of the "fallacy of misplaced concreteness"; cf. n. 45 supra.

[100] Works on metaphor include Max Black: *Models and Metaphors: Studies in Language and Philosophy* (Ithaca: Cornell University Press, 1962); Arjo Klamer (ed.): *Conversations with Economists* (Totowa, [N.J.]: Rowman & Lilienfeld, 1983); Donald N. McCloskey: *If You're So Smart: The Narrative of Economic Expertise* (Chicago/London: The University of Chicago Press, 1990); and Andrew Ortony: *Metaphor and Thought* (Cambridge/London/New York: Cambridge University Press, 1979). For a brief but incisive history of the uses of metaphor from antiquity to the present, see Mark Johnson (ed.): *Philosophical Perspectives on Metaphor* (Minneapolis: University of Minnesota Press, 1981), introd.

This topic also appears in discussions of economics, notably in Mirowski (n. 23 supra).

[101] Herbert Spencer: *The Principles of Sociology*, 3rd ed. (New York: D. Appleton and Company, 1897), vol. 1, part 2, §1, "What is a Society?", §2, "A Society is an Organism."

[102] See Schlanger (n. 160 infra).

[103] *Poetics*, 1457*b*, 1459*a*, 148*a*.

[104] That is, Aristotle held that analogy was a special kind of metaphor that involves a four-term ratio. Let the ratio be

evening : *day* :: *old age* : *life*

or

evening is to *day* as *old age* is to *life*

from which we obtain

old age is the evening of life.

Here we have a metaphor in which something (evening) is attributed to something (life) to which it does not belong. The same would be true for

evening is the old age of day.

Jevons (n. 138 infra), p. 627, gives a similar example, based on a prime minister of a state and a captain of a ship, obtaining the relation that a prime minister is captain of the state.

[105] There are a number of works dealing with rhetoric, especially in relation to the science of the seventeenth century, among them David Johnston: *The Rhetoric of Leviathan: Thomas Hobbes and the Politics of Cultural Transformation* (Princeton: Princeton University Press, 1986); Alan G. Gross: *The Rhetoric of Science* (Cambridge/London: Harvard University Press, 1990); Peter Dear (ed.): *The Literary Structure of Scientific Argument* (Philadelphia: University of Pennsylvania Press, 1991); Steven Shapin & Simon Schaffer: *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985); Marcello Pera: *Scienza e retorica* (Rome/Bari: Laterza, 1991); and M. Pera & William R. Shea (eds.): *Persuading Science: The Art of Scientific Rhetoric* (Canton, [Mass.]: Science History Publications, USA, 1991).

[106] James I: "Speech of 1603," in Charles H. McIlwain (ed.): *The Political Works of James I* (Cambridge: Harvard University Press; London: Humphrey Milford, Oxford University Press, 1918), p. 272; see Hale (n. 68 supra), p. 111.

[107] On the history of the concept of the body politic, see Hale (n. 68 supra).

[108] Ibid.

[109] For James's statement concerning the spleen, see "Speech in Star Chamber, 1616," *Political Works*, p. 343; Hale (n. 68 supra), p. 111, n. 19. See on this subject Marc Bloch: *The Royal Touch; Sacred Monarchy and Scrofula in England and France*, trans. J. E. Anderson (London: Routledge & K. Paul, 1973).
[110] Cf. §1.5 supra and §§1.7 and 1.8 infra.
[111] See the example of Desaguliers in §10 infra. On this subject see Otto Mayr: *Authority, Liberty, & Automatic Machinery in Early Modern Europe* (Baltimore: The Johns Hopkins University Press, 1986); John Herman Randall, Jr.: *The Making of the Modern Mind: A Survey of the Intellectual Background of the Present Age* (Boston: Houghton Mifflin, 1968), ch. 13.
[112] Quoted from Robert S. Hamilton's *Present Status of the Philosophy of Society* (1866) in L. L. Bernard & Jessie Bernard: *Origins of American Sociology: The Social Science Movement in the United States* (New York: Thomas Y. Crowell Company, 1943), p. 711; see p. 265 for a similar quotation concerning "the true PRINCIPIA MATHEMATICA PHILOSOPHIAE SOCIALIS." Hamilton (ibid., p. 258) believed in two sociological principles, one an analogue of the Copernican system, the other an analogue of Newton's law of universal gravity; he did not, however, fully understand Newtonian science and wrote of "centripetal" and centrifugal" forces as balanced "action" and "reaction." It is observed by the Bernards that in this respect Hamilton's law resembles the law of Carey and the law of "cosmic" attraction of Arthur Brisbane. Although Hamilton expressed admiration for Newton, and even held that he himself had propounded Copernican and Newtonian principles of sociology, he also believed that social science might become more nearly an analogue of geology than of sciences such as astronomy, physics, and chemistry. In this regard his opinion was similar to that of R.J. Wright (Bernard & Bernard, p. 306), who held that social science "ought to be compared *not* with . . . Chemistry, or Astronomy, or even Moral Philosophy, or Political Economy; but rather with . . . Geology or Metaphysics."
[113] The Newtonian style is discussed at length in my *Newtonian Revolution* (n. 46 supra) and in my article cited in n. 114 infra.
[114] For a more complete discussion of Malthus's Newtonianism, see my article in Mirowski (n. 54 supra). See, also, Anthony Flew: *Thinking about Social Thinking: the Philosophy of the Social Sciences* (Oxford: Basil Blackwell, 1985), ch. 4, §1.
[115] Thomas Robert Malthus: *An Essay on the Principle of Population as it Affects the Future Improvement of Society* (London: printed for J. Johnson, 1798). This work, published anonymously and often known as the "first essay," is readily available in two reprints, one of which, edited by Antony Flew (Harmondsworth: Penguin Books, 1970), contains also Malthus's *A Summary View of the Principle of Population* (London: John Murray, 1830), which was originally published with the author's name on the title-page. The other, without notes, but with a foreword by Kenneth E. Boulding, is entitled *Population: the First Essay* (Ann Arbor: The University of Michigan Press, 1959). The text of the second edition (1803) was so completely revised and expanded that it is generally considered "almost a new book," sometimes referred to as the "second essay." The text of this version (reprinted from the seventh edition, 1872, but without the appendices) is available as *An Essay on the Principle of Population*, intro. T.H. Hollingsworth (London: J. M. Dent & Sons, 1914 – Everyman's Library). On Malthus, see Thomas Robert Malthus: *An Essay on the Principle of Population – Text, Sources*

and Background, Criticism, ed. Philip Appleman (New York/London: W. W. Norton & Company, 1976 – Norton Critical Editions in the History of Ideas).

[116] See Flew (n. 114 supra).

[117] W. Stanley Jevons: *The Theory of Political Economy*, 2nd ed. (London: Macmillan and Co., 1879), preface; see this preface in later editions, e.g. (New York: Augustus M. Kelley, 1965 – reprint of the fifth edition, 1911), pp. xi–xiv. Jevons was defending himself against the specific charge that in his book "the equations in question continually involve infinitesimal quantities, and yet they are not treated as differential equations usually are, that is integrated" (p. 102). On Jevons's economics, see Margaret Schabas: *A World Ruled by Number: William Stanley Jevons and the Rise of Mathematical Economics* (Princeton: Princeton University Press, 1990).

[118] Mary P. Mack: *Jeremy Bentham: An Odyssey of Ideas, 1748–1792* (London: Heinemann, 1962), p. 264.

[119] This episode and its significance are discussed in I.B. Cohen: *Revolution in Science* (Cambridge, The Belknap Press of Harvard University Press, 1985), suppl. §14.1.

[120] See Shapin & Schaffer (n. 105 supra).

[121] See Mirowski (n. 23 supra); Klamer (n. 100 supra); McCloskey (n. 100 supra).

[122] Mack (n. 118 supra), pp. 275–281.

[123] Explained in Sigmund Freud: "A Note upon the 'Mystic Writing Pad,'" *The Standard Edition of the Complete Psychological Works*, vol. 19 (London: The Hogarth Press, 1961), p. 228. Two decades after *The Interpretation of Dreams*, in *Beyond the Pleasure Principle* (1920), Freud understood more clearly (as he phrased it in 1924) that "the inexplicable phenomenon of consciousness arises in the perceptual system *instead of* permanent traces" (ibid). See also ed. cit., vol. 5, p. 540, and vol. 18, p. 25; in the latter Freud noted further that this distinction had already been made by Breuer.

[124] Ibid. This pad consisted of a resin or plastic plate, covered with two sheets, one of tissue paper and the other of celluloid, on which one could write with a pointed stylus. Lifting the sheets erased the message, but Freud discovered that the erased message could actually be read in the pad's "memory." This mechanical analogue served two functions often found in the use of analogues: (1) to make his earlier hypothetical conjecture seem reasonable enough for him to set forth his ideas in full, and (2) to make his difficult concept of the structure of memory understandable and thus acceptable, to the psychoanalytic community.

[125] Freud: "Civilization and Its Discontents," *Standard Edition* (n. 123 supra), vol. 21, p. 144. See Donald M. Kaplan: "The Psychoanalysis of Art: Some Ends, Some Means," *Journal of the American Psychoanalytic Association*, 1988, **36**: 259–302, esp. 259–260.

[126] Brian Vickers: "Analogy versus Identity: The Rejection of Occult Symbolism, 1580–1680," pp. 95–163 of Brian Vickers (ed.): *Occult and Scientific Mentalities in the Renaissance* (Cambridge: Cambridge University Press, 1984).

[127] Translated by Vickers from Kepler's *Ad Vitellionem Paralipomena* (*Gesammelte Werke*, vol. 1), p. 90.

[128] Letter to Michael Maestlin, 5 March 1605, quoted in Alexandre Koyré: *The Astronomical Revolution: Copernicus – Kepler – Borelli*, trans. R. E. W. Maddison (Paris: Hermann; Ithaca: Cornell University Press, 1973), p. 252 (from Kepler's *Gesammelte Werke*, vol. 15, pp. 171–172).

[129] These "Rules" appeared in all three editions as part of the introduction of Book Three, "On the System of the World," but they were called "Rules" only in the second (1713) and third (1726) editions.

[130] Laplace's *System of the World*, vol. 2, p. 316, as in Jevons (n. 138 infra), p. 638.

[131] Charles Darwin: *The Origin of Species* (London: John Murray, 1859; reprint, Cambridge: Harvard University Press, 1964), ch. 3, p. 63. This is a case of analogy rather than of generalization because it extends a property observed in one group of entities (humans) to other groups of different entities (plants and animals), whereas a generalization extends a property of some members of a given class to other (or, even, to all) members of that class, as in the generalization that all men are mortal.

Darwin drew on the argument from analogy in other parts of the *Origin*. The concept of natural selection was introduced in analogy with man's process of "artificial" selection in breeding pigeons, horses, dogs, and various ornamental and useful plants. A classic use of analogy, as opposed to generalization, occurs in the final chapter of the *Origin*, in Darwin's presentation of the theory of "common descent." He first concluded that all animals had "descended from at most only four or five progenitors, and plants from an equal or lesser number." This led him to remark, "Analogy would lead me one step further, namely, to the belief that all animals and plants have descended from one prototype." He was aware, as he wrote, that "analogy may be a deceitful guide." Yet he found the evidence for common descent to be very persuasive, noting that "all living things have much in common, in their chemical composition, their germinal vesicles, their cellular structure, and their laws of growth and reproduction." This evidence justified his inference "from analogy that probably all the organic beings that have ever lived on this earth have descended from one primordial form, into which life was first breathed."

[132] See Nagel (n. 136 infra), pp. 107–110.

[133] James Clerk Maxwell: "On Faraday's Lines of Force", in W. D. Niven (ed.): *The Scientific Papers of James Clerk Maxwell* (Cambridge: Cambridge University Press, 1890; reprint, New York: Dover Publications, 1965), vol. 1, p. 156.

[134] This was the occasion for Maxwell to make what may be considered the classic statement about the use of what he called "physical analogies" in science. According to Maxwell, "physical analogies" provide a means "to obtain physical ideas without adopting a physical theory." Ernest Nagel (n. 136 infra, p. 109) has explained that Maxwell meant that he could obtain physical ideas without invoking a "theory formulated in terms of some particular model of physical processes." In other words, by "physical analogies" he implied no more than "that partial similarity between the laws of one science and those of another which makes each of them illustrate the other."

[135] On this point, see especially articles and books by Mirowski.

[136] Ernest Nagel: *The Structure of Science: Problems in the Logic of Scientific Explanation* (New York/Burlingame: Harcourt, Brace & World, 1961), pp. 107–117.

[137] See Maxwell (n. 133 supra). See also J. Robert Oppenheimer, "Analogy in Science," *The American Psychologist* 1956, **11**: 127–135, an address to psychologists in which the physicist J. Robert Oppenheimer stated boldly and unequivocally that "analogy is indeed an indispensable and inevitable tool for scientific progress" (p. 129). He at once narrowed the sense of his assertion, trying to make clear what he meant. "I do not mean metaphor," he added, "I do not mean allegory; I do not even mean similarity." Rather, he intended "a special kind of similarity which is the similarity of structure, the simi-

larity of form, a similarity of constellation between two sets of structures, two sets of particulars, that are manifestly very different but have structural parallels."

138 W. Stanley Jevons: *The Principles of Science: A Treatise on Logic and Scientific Method* (2nd and final edition, reprint, New York: Dover Publications, 1958), p. 631.

139 Ibid.

140 Ibid., p. 632.

141 Jevons (1965; n. 117 supra), p. 102. It was even suggested by Jevons (n. 138 supra, p. 633), on the authority of Lacroix, that "the discovery of the Differential Calculus was mainly due to geometrical analogy, because mathematicians, in attempting to treat algebraically the tangent of a curve, were obliged to entertain the notion of infinitely small quantities." See Schabas (n. 117 supra), pp. 84–88, "Mechanical Analogies."

142 Jevons (1965; n. 117 supra), p. 105.

143 Léon Walras: "Economique et mécanique", *Bulletin de Société Vaudoise des Sciences Naturelles*, 1909, **45**: 313–325; Mirowski & Cook (n. 48 supra), pp. 189–224.

Francis Ysidro Edgeworth proposed the same kind of analogy between his "mathematical psychics" (as he called his brand of economics) and mathematical physics, declaring that "every psychical phenomenon is the concomitant, and in some sense the other side of a physical phenomenon." He had no doubt that "'Mécanique Sociale' may one day take her place along with 'Mécanique Céleste,' throned each upon the double-sided height of one maximum principle, the supreme pinnacle of moral as of physical science." See F. Y. Edgeworth's *Mathematical Psychics: An Essay on the Application of Mathematics to the Moral Sciences* (London: C. Kegan & Co., 1881), esp. pp. 9, 12.

144 See Claude Ménard (n. 97 supra).

145 Vilfredo Pareto: "On the Economic Phenomenon: A Reply to Benedetto Croce," translated from Italian by F. Priuli in Alan Peacock, Ralph Turvey, & Elizabeth Henderson (eds.): *International Economic Papers*, vol. 3 (London: Macmillan and Company, 1953), p. 185. For a discussion of Pareto's point of view see Mirowski (n. 23 supra), pp. 221–222; also Bruna Ingrao: "Physics and Pareto's Economics," to be published in Mirowski (n. 54 supra).

146 Vilfredo Pareto: *Sociological Writings*, ed. S. E. Finer, trans. Derick Mirfin (Oxford: Basil Blackwell, 1966), pp. 103–105, selected from Pareto's *Cours d'économic politique* (Lausanne, 1898), vol. 2, §§580, 588–590.

147 Ibid. See also Bruna Ingrao: "L'analogia meccanica nel pensiero di Pareto," in G. Busino (ed.), *Pareto oggi* (Bologna: Il Mulino, 1991); and her chapter cited in n. 145 supra.

148 J.E. Cairnes: *The Character and Logical Method of Political Economy* (New York: Harper & Bros., 1875) p. 69. See Mirowski (n. 23 supra), p. 198.

149 Leonard Huxley (ed.): *Life and Letters of Thomas Henry Huxley*, vol. 1 (London: Macmillan, 1900), p. 218.

150 See Mirowski (n. 23 supra), pp. 218–219, 287; William Stanley Jevons: *The Principles of Economics* (London: Macmillan and Co., 1905), p. 50; Jevons: *The Theory of Political Economy* (1965; n. 117 supra), pp. 61–69; Jevons: *The Principles of Science* (n. 138 supra), pp. 325–328; Jevons: *Papers and Correspondence of William Stanley Jevons*, vol. 7, ed. R.D. Collison Black (London: Macmillan, in association with the Royal Economic Society, 1981), p. 80.

151 Léon Walras: *Elements of Pure Economics*, trans. William Jaffé (Homewood [Ill.]:

Richard D. Irwin; London: George Allen & Unwin; reprint, Philadelphia: Orion Editions, 1984), Preface to the fourth edition, pp. 47–48; also p. 71.

152 Ménard (n. 97 supra).

153 Albert Jolink: "'Procrustean Beds and All That': The Irrelevance of Walras for a Mirowski-Thesis," to appear in 1993 in a special issue of *History of Political Economy*, edited by Neil de Marchi, containing papers presented at a symposium (held at Duke University in April 1991) on Mirowski's *More Heat than Light.*

154 Pareto (n. 146 supra), *Sociological Writings*, p. 104; *Cours*, vol. 2, §592; see the article by Bruna Ingrao, cited in n. 145 supra.

155 Mirowski (n. 23 supra), pp. 222–231. Extracts from this manuscript, preserved in the Sterling Library, Yale University, are quoted by Mirowski (pp. 228–229, 409 n. 5).

156 See, e.g., Hal Varian's review of Mirowski's *More Heat than Light* in the *Journal of Economic Literature*, 1991, **29**: 595–596.

157 This was part of an article on "Distribution and Exchange" in the *Economic Journal* for March 1898 and reprinted in A. C. Pigou (ed.): *Memorials of Alfred Marshall* (London: Macmillan and Co., 1925), pp. 312–318.

158 Marshall was repeating here the sentiments he had expressed in his inaugural lecture as professor of economics at Cambridge University, printed in Pigou (n. 157 supra; see §1.8 infra).

159 I have chosen these three organismic sociologists – one Russian, one Austrian, and one French – because their writings exemplify the main issues in the interactions of the natural and the social sciences. There are many others whose writings show the same features, notably the Germany biologist Oscar Hertwig and the Italian sociologist Corrado Gini.

160 On organismic sociology see F. W. Coker: "Organismic Theories of the State: Nineteenth Century Interpretations of the State as Organism or as Person," *Studies in History, Economics and Public Law* (New York: Columbia University, 1910), vol. 38, no. 2, whole number 101; Ludovic Gumplowicz: *Geschichte der Staatstheorien* (Innsbruck: Universitäts-Verlag Wagner, 1926); Sorokin (n. 55 supra), ch. 4, "Biological Interpretation of Social Phenomena"; Werner Stark (n. 74 supra), part 1, "Society as an Organism"; Judith Schlanger: *Les métaphores de l'organisme* (Paris: Librairie Philosophique J. Vrin, 1971).

Some further major secondary sources on the subject of organismic sociology are: Arnold Ith: *Die menschliche Gesellschaft als sozialer Organismus: Die Grundlinien der Gesellschaftslehre Albert Schäffles* (Zurich/Leipzig: Verlag von Speidel & Wurzel, 1927); Niklas Luhmann: *Die Wirtschaft der Gesellschaft* (Frankfurt: Suhrkamp, 1988); N. Luhmann: *Die Wissenschaft der Gesellschaft* (Frankfurt: Suhrkamp, 1990); D. C. Phillips: "Organicism in the Late 19th and Early 20th Centuries," *Journal of the History of Ideas*, 1970, **31**: 413–432; E. Scheerer: "Organismus," pp. 1330–1358 of J. Ritter (ed.): *Historisches Wörterbuch der Philosophie* (Darmstadt: Wissenschaftliche Verlagsgesellschaft, 1971).

Still valuable as sources of information are certain older works, notably Ezra Thayer Towne: *Die Auffassung der Gesellschaft als Organismus, ihre Entwicklung und ihre Modifikationen* (Halle: Hofbuchdruckerei von C. A. Kammerer & Co., 1903); Erich Kaufmann: *Über den Begriff des Organismus in der Staatslehre des 19. Jahrhunderts* (Heidelberg: C. Winter, 1908).

None of these works, however, pays any attention to the specific relation of these nineteenth-century organismic social scientists to the currents of discovery in the life sciences in their own time.

[161] Alfred Marshall: *The Present Position of Economics: An Inaugural Lecture Given in the Senate State House at Cambridge, 24 February, 1885* (London: Macmillan and Co., 1885), pp. 12–14; this lecture is reprinted in Pigou (n. 157 supra), pp. 152–174.

[162] As most people are aware (because of the interest which Sigmund Freud and Josef Breuer had in this subject), hysteria was a major focus of psychiatric attention in the nineteenth century. An example of hysteria has been introduced in §1.4.

[163] As explained in the Preface to this volume, there is no attempt to discuss all aspects of biological science that have interacted with the natural sciences. I have not dealt with the subject of Darwinian evolution because this interaction is far too complex to be considered in a summary fashion and because it is already the subject of a vast literature that is a continuing part of the current Darwin "industry." Some major aspects of this subject, with special reference to America, are developed in an important way in Robert Richards's *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior* (n. 81 supra), a work that can be especially commended for its methodological approach. Among recent contributions to this general area are Carl N. Degler: *In Search of Human Nature: The Decline and Revival of Darwinism in American Social Thought* (New York/Oxford: Oxford University Press, 1991), and Dorothy Ross: *The Origins of American Social Thought* (Cambridge/New York: Cambridge University Press, 1991). Also worthy of mention is Cynthia Eagle Russett: *Darwin in America: The Intellectual Response, 1865–1912* (San Francisco: W. H. Freeman, 1976).

[164] Auguste Comte: *The Foundations of Sociology*, ed. Kenneth Thompson (New York: John Wiley & Sons, 1975), p. 142. The text is taken from the English translation of Auguste Comte's *System of Positive Polity*, 4 vols (London: Longmans Green, 1877), translated by a group of scholars from *Système de politique positive* (Paris, 1848–1854), vol. 2, pp. 367–382.

Comte believed that Broussais's principle of continuity was especially important in considering the "opposite" mental states of "reason and madness." If the mind surrendered itself to the sense impressions of the external world "with no due effort of the mind within," the result would be "pure idiocy." Madness, in all its intermediate degrees results from the relative failure of the "apparatus of meditation" to "correct the suggestions made by the apparatus of observation." This phenomenon could, he asserted, be studied better in Cervantes's *Don Quixote* "than in any treatise of biology." It could also be traced to "the great principle of Broussais" and could then be "applied to society" as Comte had "now done for the first time." See Comte's *Cours de philosophie positive* (Paris, 1830–1842), quoted in Gertrud Lenzer (ed.): *Auguste Comte and Positivism: The Essential Writings* (New York/Evanston/San Francisco: Harper & Row, 1975), p. 191, taken from *The Positive Philosophy of Auguste Comte*, trans. [and condensed by] Harriet Martineau (London: Longmans, Green, 1853), book 5, ch. 6.

[165] In Lenzer (n. 164 supra), p. 191.

[166] See Edmund Beecher Wilson: *The Cell in Development and Heredity* (New York: The Macmillan Company, 1896; reprint of 3rd ed., New York: The Macmillan Company, 1934), esp. pp. 1–2. Although preliminary steps can be traced to earlier scientists, it was not until the 1840s – largely as a result of the work of J.M. Schleiden and espe-

cially Theodor Schwann – that biologists generally began to give cell theory full serious consideration.

[167] In nineteenth-century thought the principle of division of labor was usually credited to Adam Smith, who displayed it in a dramatic fashion in the opening pages of *The Wealth of Nations*, even though there were other contenders for the invention, including both Benjamin Franklin and Sir William Petty.

[168] This difference is discussed by all organicist sociologists, e.g., Spencer, Lilienfeld, Schäffle.

[169] René Worms called attention to two limitations of this analogy which had been stressed by Herbert Spencer. The first is that, although each individual in the social organism has consciousness, in the animal organism only the organism as a whole, and not the individual cell, has this property. The second: in the social organism the purpose of society, or the organism as a whole is to sustain the lives of the individuals, whereas in the animal or plant the lives of the individual cells serve to support the life of the organism as a whole. Despite these dissimilarities, the cell theory seemed to provide nature's own model on the microscopic scale for the study of human societies, much as the social behavior of ants has done in our own days.

[170] As in societies, the development of the embryo produces special cells and groups of cells with forms and structures suited to their function. This concept of "division of labor," as we have seen (n. 61 supra), originated in social science, then was transferred to the life science and finally migrated back to the social sciences. This transfer is the subject of chapter 10 infra.

[171] On von Baer see the article by Jane Oppenheimer in the *Dictionary of Scientific Biography*, vol. 1 (New York: Charles Scribner's Son, 1970), pp. 385–389.

[172] See Steven J. Gould: *Ontogeny and Phylogeny* (Cambridge: The Belknap Press of Harvard University Press, 1977).

[173] In this section I have not dealt particularly with Herbert Spencer, although he is perhaps the most important of all the organicist social scientists. One reason is that, unlike many other organic sociologists, he did not concentrate attention on bio-medical discoveries relating to the cell theory, although he did make use of cell biology in his writings on sociology. Some of Spencer's uses of biological science in relation to sociology are discussed in §1.4 supra and §1.8 infra. On Spencer's use of analogies, see ch. 8 infra. On the subject of Spencer and sociology, see Richards (n. 81 supra). Cf. also n. 208 infra.

[174] Herbert Spencer: *First Principles* (London: Williams and Norgate, 1862), §119. Spencer evidently learned this law from William Carpenter; see Richards (n. 81 supra), p. 269. Richards observes that Carpenter thought that von Baer's law (that "a heterogeneous structure arises out of one more homogeneous") had great generality. Carpenter wrote that "if we watch the progress of evolution [i.e., embryonic development], we may trace a correspondence between that of the germ in its advance towards maturity, and that exhibited by the permanent condition of the races occupying different parts of the ascending scale of creation."

[175] Herbert Spencer, "Reasons for Dissenting from the Philosophy of M. Comte," *Essays: Scientific, Political, and Speculative*, vol. 2 (New York: D. Appleton and Company, 1896), pp. 118–144.

[176] Ibid., pp. 137–138. It should be noted also that cellular embryology reinforced another

principle of organismic sociology. Embryologists revealed that as an individual organism progresses through more and more complex forms, the component cells exhibit structures suitably adapted for their special function, that is, they show the form necessary for the "division of labor." Spencer held that even before encountering von Baer's "law," he had begun to conceive of both "the development of an individual organism and the development of the social organism" as an advance from "independent parts to mutually-dependent unlike parts – a parallelism implied by Milne-Edwards' doctrine of the 'physiological division of labor.'"

177 For Virchow, the concept of the "cell state" was particularly significant because there was always a close parallel between his "biological views and his liberal political opinions." See Owsei Temkin: "Metaphors of Human Biology," in Robert C. Stauffer (ed.): *Science and Civilization* (Madison: University of Wisconsin Press, 1949), p. 172. Temkin is summarizing Ernst Hirschfeld, "Virchow," *Kyklos: Jahrbuch des Instituts für Geschichte der Medizin an der Universität Leipzig*, 1929, **2**: 106–116. See also Erwin H. Acherknecht: *Rudolf Virchow: Doctor, Statesman, Anthropologist* (Madison: The University of Wisconsin Press, 1953); reprint (New York: Arno Press, 1981).

178 Rudolf Virchow: *Cellular Pathology As Based upon Physiological and Pathological Histology*, trans. Frank Chance (New York: Robert M. DeWitt, 1860), p. 40; also (London: John Churchill), pp. 13–14.

179 Virchow, we may note, was not the only nineteenth-century biologist to use social analogies in scientific discourse. Thomas Henry Huxley made use of a social analogy in describing the sponge, which – he said – represented a kind of sub-aqueous city, "in which the people are arranged about the streets and roads, in such a manner, that each can easily appropriate his food from the water as its passes along." This is an example of the use of analogy to illustrate a scientific concept, making such a concept easier to visualize or to understand.

180 See Temkin (n. 177 supra), p. 175.

181 Ibid.

182 Paul von Lilienfeld, or Paul de Lilienfeld, or Pavel Fedorovich Lilienfeld-Toailles, or Pavel Fedorovich Lilienfel'd Toal' (1829–1903), was a Russian civil servant who held responsible government posts and whose avocation was sociology. He published a book in Russian on the elements of political economy in 1860 under the pseudonym "Lileyewa." Another work, appearing first in 1872 in Russian, under the initials P. L., bore the title, *Thoughts on the Social Science of the Future*, which was expanded into a five-volume German version, *Gedanken über die Socialwissenschaft der Zukunft* (vols. 1–4: Mitau: E. Behre's Verlag, 1873–1879; vol. 5: Hamburg: Gebr. Behre's Verlag; Mitan: E. Behre's Verlag, 1881). Of particular importance are *La pathologie sociale* (Paris: V. Giard & E. Brière, 1896) and *Zur Vertheidigung der organischen Methode in der Sociologie* (Berlin: Druck und Verlag von Georg Reimer, 1898). In 1897–1898 Lilienfeld was president of the Institut International de Sociologie. See Otto Henne am Rhyn: *Paul von Lilienfeld* (Gdansk, Leipzig, Vienna: Carl Hinstorff's Verlagsbuchhandlung [n.d.] – Deutsche Denker und ihre Geistesschöpfungen, ed. Adolf Hinrichsen, vol. 6). For further bibliography related to Lilienfeld, see Howard Becker: "Lilienfeld-Toailles, Pavel Fedorovich," *Encyclopaedia of the Social Sciences*, vol. 9 (New York: The Macmillan Company, 1933, 1937), p. 474. See also n. 160 supra for a list of publications relating to organismic sociology. (The first four volumes of the *Gedanken* in one of the sets in

the Harvard University Library contain book plates indicating that they were "bought with the income from the bequest of James Walker . . . former president of Harvard College; 'preference being given to works in the intellectual and moral sciences.' ")

183 Trans. from *Gedanken*, vol. 1, p. v.

184 Trans. from *Pathologie*, p. xxii.

185 Ibid.

186 Ibid., p. 8.

187 Ibid., pp. 8–11. Lilienfeld was noted in his own time for his discussion of social diseases that were analogues of diseases of the nervous system, particularly psychological disorders. We have seen (§1.4 supra) an example of his suggestion of a parallel between medical and social disorders in the social analogue of the intellectual and moral state of women suffering from hysteria.

188 Ibid., pp. 20–21.

189 Ibid., p. 21.

190 Ibid., p. 24.

191 Ibid., pp. 46–47.

192 Ibid., p. 307.

193 *Bau und Leben des socialen Körpers: Encyclopädischer Entwurf einer realen Anatomie, Physiologie und Psychologie der menschlichen Gesellschaft mit besonderer Rücksicht auf die Volkswirthschaft als socialen Stoffwechsel*, 4 vols. (Tübingen: H. Laupp'sche Buchhandlung, 1875–1878).

Albert Eberhard Friedrich Schäffle (1831–1903), a German sociologist and economist, was a professor at the University of Tübingen, later moving to the University of Vienna. He was, for a while, a member of the Austrian cabinet. He edited a journal entitled *Zeitschift für die Gesamte Staatswissenschaft*. He envisioned a "rational social state," a kind of utopian blend of capitalism and socialism. He was known in his own times primarily for his exposition of organismic social theory, especially his use of specific biological analogies. See the article on him by Fritz Karl Mann in *Encyclopaedia of the Social Sciences*, ed. Edwin R.A. Seligman (New York: The Macmillan Company, 1934), vol. 13, pp. 562–563. There is no biography of Schäffle in the more recent *International Encyclopedia of the Social Sciences*. See Arnold Ith (n. 160 supra) and Stark (n. 74 supra), pp. 62–72.

194 Schäffle (n. 193 supra) vol. 1, p. 286; see Stark (n. 74 supra), p. 63. The extracts from Schäffle are quoted from Stark's translation.

195 Schäffle, vol. 1, p. 286; Stark, pp. 63–64.

196 Preface to Lilienfeld's *La pathologie sociale* (n. 182 supra), p. vii; cf. Stark, p. 63.

197 Schäffle, vol. 1, p. 286; Stark, p. 64.

198 Schäffle, vol. 1, p. 33; Stark, p. 66.

199 Schäffle, vol. 1, p. 57; Stark, p. 67.

200 Schäffle, vol. 1, p. 324; Stark, p. 67.

201 Schäffle, vol. 1, p. 335; Stark, p. 67.

202 Schäffle, vol. 1, pp. 327, 329; Stark, p. 68.

203 Schäffle, vol. 1, p. 94; Stark, p. 68.

204 René Worms (1869–1926), a French sociologist, was educated at the Ecole Normale Supérieure. In 1893 he founded both the Paris-based Institut International de Sociologie and the *Revue Internationale de Sociologie*. He also founded and edited a series of fifty

books on sociological subjects by authors from many countries. He was known in his lifetime particularly for his views concerning the interrelations among "the three disciplines of psychology, social psychology, and sociology." See the biography and critical analysis by Terry N. Clark in *International Encyclopedia of the Social Sciences*, ed. David L. Sills, vol. 16, pp. 579–581 (New York: The Macmillan Company & The Free Press, 1968).

An account of the life and career of René Worms may be found in an article by V.D. Sewny in the *Encyclopaedia of the Social Sciences*, vol. 15 (New York: The Macmillan Company, 1934), pp. 498–499. See also Stark (n. 74 supra).

205 Worms (n. 87 supra), p. 43.

206 René Worms: *Philosophie des sciences sociales* (Paris: V. Giard & E. Brière, 1903), vol. 1, p. 53.

207 Ibid., chs. 2, 3.

208 See, especially, Derek Freeman, "The Evolutionary Theories of Charles Darwin and Herbert Spencer," *Current Anthropology*, 1974, **15**: 211–237. Cf. also n. 173 supra.

209 *Nature*, 1982, **296**: 686–687.

210 J.W. Burrow: *Evolution and Society: A Study in Victorian Social Theory* (Cambridge: The University Of Cambridge Press, 1970), p. 182.

211 Peel (n. 81 supra), p., 174, including a quotation from Spencer's *Social Statics*.

212 The organic metaphors predominate in many essays (notably "The Social Organism" [1860]) and in his books, especially *Social Statics* (1850), *The Study of Sociology* (1873), and *The Principles of Sociology* (1876). See Peel (n. 81 supra), ch. 7, esp. p. 174.

213 Quoted in Peel (n. 81 supra), p. 179.

214 Ibid., p. 178.

215 I have not felt the need to make a parade here of the mismatched homologies that appear in the writings of Lilienfeld, Schäffle, Worms, and Spencer (see §1.4 supra), because my aim has been to examine the historical use of analogies rather than merely to call attention to their extravances (as has been done in §1.4 supra).

216 Michel Foucault: *Power/Knowledge: Selected Interviews and Other Writings, 1972–1977*, ed. Colin Gordon (Brighton: Harvester Press, 1980), p. 151.

217 See Small and Vincent (n. 18 supra).

218 Bryan S. Turner: *The Body and Society: Explorations in Social Theory* (Oxford: Basil Blackwell, 1984), pp. 49–50; Louis Wirth: "Clinical Sociology," *American Journal of Sociology*, 1931, **37**: 49–66.

219 L.J. Henderson: "Physician and Patient as a Social System," *New England Journal of Medicine*, 1935, **51**: 819–823; "The Practice of Medicine as Applied Sociology," *Transactions of the Association of American Physicians*, 1936, **51**: 8–15. These and other papers of Henderson on similar subjects have been edited with an important introductory statement by Bernard Barber: *L.J. Henderson on the Social System: Selected Writings* (Chicago: University of Chicago Press, 1970).

See, on this subject, Talcott Parsons: *The Social System* (Glencoe, Ill.: Free Press, 1951) and "The Sick Role and the Role of the Physician Reconsidered," *Milbank Memorial Fund Quarterly*, 1975, **53**: 257–278.

220 Marie-Jean-Antoine-Nicolas Caritat, Marquis de Concorcet: *Esquisse d'un tableau historique des progrès de l'esprit humain* (Paris: Agasse, 1795); also Baker (n. 29 supra), pp. 348–349, 368–369.

221 In later editions Malthus, attempting to lessen the gloomy prospect he had set forth, introduced the power of "moral restraint" as a factor in population control. See n. 115 supra.

222 David Hume: "Of the Populousness of Ancient Nations," vol. 1 of his *Essays, Moral, Political, and Literary* (Edinburgh: R. Fleming and A. Alison for A. Kincaid, 1742), p. 376. See Catherine Gallagher: "The Body versus the Social Body," pp. 83–106 of Catherine Gallagher & Thomas Laqueur (eds.): *The Making of the Modern Body: Sexuality and Society in the Nineteenth Century* (Berkeley/Los Angeles: University of California Press, 1987).

223 Carey (n. 43 supra); see §1.4 supra.

224 See Sorokin (n. 55 supra) and Stark (n. 74 supra).

225 *The Spirit of the Laws*, trans. Thomas Nugent (revised ed., London: George Bell and Sons, 1878; reprint, New York: Hafner Press, 1949), bk. 3, §7, "The Principle of Monarchy."

226 On this score see Henry Guerlac: "Three Eighteenth-Century Social Philosophers: Scientific Influences on their Thought," *Daedalus*, 1958, **87**: 6–24; reprinted in Henry Guerlac: *Essays and Papers in the History of Modern Science* (Baltimore: The Johns Hopkins University Press, 1977), pp. 451–464.

227 Adam Smith: *An Inquiry into the Nature and Causes of the Wealth of Nations* (Oxford: Oxford University Press, 1976 – The Glasgow Edition of the Works and Correspondence of Adam Smith, II), bk. 1, ch. 7, p. 15 (§15). The "Cannan edition" – Adam Smith: *An Inquiry into the Causes of the Wealth of Nations*, ed. Edwin Cannan (London: Methuen & Co., 1904; reprint, Chicago: The University of Chicago Press, 1976; reprint New York: Modern Library, 1985) – is easier to read and has the advantage of useful postils. A postil (1976 ed., p. 65; 1985 ed., p. 59) repeats the message: "Natural price is the central price to which actual prices gravitate."

228 Adam Smith: *Essays on Philosophical Subjects*, ed. W.P.D. Wightman & J.C. Bryce (Oxford: Oxford University Press, 1980 – The Glasgow Edition of the Works and Correspondence of Adam Smith), vol. 3, pp. 33–105, "The History of Astronomy."

229 Ménard (n. 97 supra; 1988).

230 For details see my *Introduction to Newton's 'Principia'* (Cambridge: Harvard University Press; Cambridge: Cambridge University Press, 1971), ch. 2, §1. Newton referred to the inertial property of bodies as both a "vis inertiae" or "force of inertia" and "inertia." For him this was an "internal" rather than an "external" force and so could not – of and by itself – alter a body's state of rest or of motion.

231 On Darwin and Lyell see Mayr (n. 40 supra); the details of Darwin's transformation are discussed, along with other examples, in my *Newtonian Revolution: With Illustrations of the Transformation of Scientific Ideas* (New York/Cambridge: Cambridge University Press, 1980). This topic is explored also in my forthcoming *Scientific Ideas* (New York: W. W. Norton & Company, 1994).

232 Mirowski (n. 23 supra, pp. 241–254) has documented the way in which Joseph Bertrand and Hermann Laurent faulted Walras for his mathematical physics, as Laurent and Vito Volterra later faulted Pareto.

233 *Journal of Economic Literature*, 1991, **29**: 595–596.

234 But even a severe critic like Hal R. Varian does admit that Mirowski's "thorough search of the writings of Walras, Jevons, Fisher, Pareto, and other neoclassicals . . . has

established, to almost anyone's satisfaction that they recognized that 'utility' had some features in common with the then-current notions of 'energy'."

235 A. Lawrence Lowell: "An Example from the Evidence of History," in Harvard Tercentenary Conference of Arts and Sciences (1936): *Factors Determining Human Behavior* (Cambridge: Harvard University Press, 1937), pp. 119–132.

236 Of course, one reason why an analogy may be inappropriate is that it is based on mismatched homology. Another reason might be that the analogy did not advance the subject to the same degree as a rival one.

237 Henry Guerlac once described it as one of the worst in the English language. Jean T. Desaguliers: *The Newtonian System of the World, the Best Model of Government* (Westminster: A. Campbell, 1728).

238 John Craig: *Theologiae Christianae Principia Mathematica* (London: impensis Tomothei Child, 1699). A translation of some major extracts by Anne Whitman is published (without the translator's name) as "Craig's Rules of Historical Evidence," *History and Theory: Studies in the Philosophy of History*, Beiheft 4 (The Hague: Mouton, 1964). Craig once suggested to Newton a minor modification of the *Principia*; see I. B. Cohen: "Isaac Newton, the Calculus of Variations, and the Design of Ships," pp. 169–187 of Robert S. Cohen, J.J. Stachel, & M.M. Wartofsky (eds.): *For Dirk Struik: Scientific, Historical, and Political Essays in Honor of Dirk J. Struik* (Dordrecht/Boston: D. Reidel Publishing Company, 1974 – Boston Studies in the Philosophy of Science, vol. 15).

239 For two centuries and more, Craig's book and its Newton-like laws have usually been presented as an example of the kind of aberration to which Newtonian science may lead. His whole book can, in fact, be considered an extended example of inappropriate analogy. Yet a recent study by Stephen Stigler ("John Craig and the Probability of History: From the Death of Christ to the Birth of Laplace," *Journal of the American Statistical Association*, 1986, **81**: 879–887) has shown that Craig made a serious contribution to applied probability, "that his formula for the probability of testimony was tantamount to a logistic model for the posterior odds."

240 I have not attempted to rewrite the history of this subject, displayed in many monographs, beginning with Richard Hofstadter: *Social Darwinism in American Thought* (Philadelphia: University of Pennsylvania Press, 1944; rev. ed., Boston: Beacon Press, 1955). Some more recent works are Degler (n. 163 supra) and Robert C. Bannister: *Social Darwinism: Science and Myth in Anglo-American Social Thought* (Philadelphia: Temple University Press, 1979); Howard L. Kaye: *The Social Meaning of Modern Biology: From Social Darwinism to Social Biology* (New Haven: Yale University Press, 1984); Peter J. Bower: *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900* (Baltimore: Johns Hopkins University Press, 1983).

241 Michael Ruse: "Social Darwinism: Two Roots," *Albion*, 1980, **12**: 23–36.

242 Spencer: "The Study of Sociology," No. XVI, "Conclusion," *Contemporary Review*, 1873, **22**: 663–677, esp. p. 676.

243 Stephen Jay Gould: "Shoemaker and Morning Star: A Visit to the Great Reminder reveals some Painful Truths carved in Stone," *Natural History*, December 1990, pp. 14–20, esp. p. 20.

244 Gould's analogy rests on an imperfect homology. Lamarckian evolution in biology implies not only that each individual may modify his or her inheritance but that such modifications are transmitted to one's offspring. Consider a catastrophe in which all

material culture and all humans over the age of three would be destroyed. In a Lamarckian social world homologous with a Lamarckian biological world, the surviving individuals would have inherited the technological knowledge and skills acquired by centuries of evolutionary development. In the world of nature and of man, however, this would not be the case, as Gould is aware.

245 Additionally Gould alleges that the Lamarckian mode "of cultural transmission" is responsible for "all the ills of our current environment crisis" as well as "the joys of our confidently growing children."

246 This example was brought to my attention by Neil Niman at a symposium on Natural Images in Economics. He, however, treats this episode in a wholly different way from mine. See his paper in Mirowski (n. 54 supra).

247 Armen A. Alchian: "Uncertainty, Evolution and Economic Theory," *Journal of Political Economy*, 1950, **57**: 211–221.

248 Edith Tilton Penrose: "Biological Analogies in the Theory of the Firm," *The American Economic Review*, 1952, **42**: 804–819, esp. p. 805.

249 Ibid., p. 807.

250 Ibid., p. 812.

251 Armen A. Alchian: "Biological Analogies in the Firm: Comment," *The American Economic Review*, 1953, **43**: 600–603.

252 Edith T. Penrose: "Rejoinder," ibid., pp. 603–609. Penrose quotes from Alchian's original article to the effect that the "suggested approach embodies the principles of biological evolution and natural selection."

253 William F. Ogburn: *Social Change with Respect to Culture and Original Nature*, 2d ed. (New York: Viking Press, 1950), Supplement.

254 Ménard (n. 97 supra; 1988), p. 91.

255 *Equality of Educational Opportunity*, 2 vols. (Washington, D.C.: Office of Education – U.S. Department of Health, Education, and Welfare – U.S. Government Printing Office, 1966).

256 Quoted in Frederick Mosteller & Daniel P. Moynihan (eds.): *On Equality of Educational Opportunities: Papers Deriving from the Harvard University Faculty Seminar on the Coleman Report* (New York: Random House, 1972), pp. 4–5.

257 See the editors' discussion of crude and refined statistics (ibid., pp. 12–14) and also ch. 11 by Christopher S. Jencks on "The Quality of the Data Collected by *The Equality of Educational Opportunity* Survey." The second volume of the Coleman Report consisted of 548 pages of tables of means, standard deviations, and correlation coefficients, as a complement to the 373 pages of the first volume.

258 In Mosteller & Moynihan (n. 256 supra), p. 33, there is a critique of the statistics and their interpretation. Chapter Four, by James S. Coleman, is on "The Evaluation of Equality of Educational Opportunity."

259 Mosteller & Moynihan (n. 256 supra), p. 32.

IAN HACKING

2. HOW NUMERICAL SOCIOLOGY BEGAN BY COUNTING SUICIDES: FROM MEDICAL PATHOLOGY TO SOCIAL PATHOLOGY*

Usually when collective tendencies or passions are spoken of, we tend to regard these expressions as mere metaphors and manners of speech with no real signification but a sort of average among a certain number of individual states. They are not considered as things, forces *sui generis* which dominate the consciousness of single individuals. None the less this is their nature, as is brilliantly shown by the statistics of suicide.[1]

Those are the familiar words of Emile Durkheim's *Suicide*, published in 1897. I am concerned not with the truth of his opinion, but with how it became possible to think those novel thoughts. We here have the idea of laws, acting upon individuals, demonstrated by statistics, and not arising simply from facts about individuals by interaction and composition. These laws are autonomous, holistic, and not merely the summary of the determined choices of members of the population. This idea was unthinkable at the start of the nineteenth century. At that time one had the gaunt universal determinism of a Laplace, or the organic vitalism of a Bichat. The one said that statistical phenomena arise from minute fully deterministic causes. When we cannot rise above probabilities, it is because of our ignorance. The other denied determinism in the biological sphere, but made no space for statistics: the physician and the histologist must understand the vital workings of an individual, who might be typical of the species, but whose functioning could not be summarized by an average. Yet by the end of the century there was a family of conceptions, in numerous fields, akin to Durkheim's. Durkheim illustrates a phenomenon that I call the taming of chance. Note that Bichat and Laplace were equally frightened by chance!

This taming goes hand in hand with the erosion of universal determinism and leads to the greatest metaphysical revolution of the modern world, the discovery made by the physicists of the twentieth century, that we live in a universe of chance. I am not saying that Durkheim believed we live in such a universe. On the contrary, he was a determinist, prepared to regard laws of populations as binding, necessary, inescapable.

Where a previous generation had insisted that every statistical law be founded on an underlying causal microstructure, he was positively

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 101–133.

opposed to such an idea. His laws of society were irreducible, although their manifestations are statistical. That was one stage in what I just called the erosion of determinism. The word "erosion" is chosen with due deliberation. The mountain is still there when it is being eroded, but were it not being eroded, it would never in the end disappear into the dust of the plain.

Durkheim and his social statistics represent only one aspect of this erosion. Moreover it is not the man, Durkheim, who is the object of my study. Nor am I preoccupied by the question, how did numerical and statistical sociology come into being? I wish instead to describe how the details of that development contributed to the anti-deterministic transformation in Western thought. I do not imply that the participants, such as Durkheim, were aware of their contribution to that mutation, or would have welcomed any kind of indeterminism. I recount what one might call the deterministic sources of antideterminism. We cannot understand the full sweep of such events without attending to a great many of the winds and rivulets that did the eroding. The present essay examines only one rivulet. One point of collaborative endeavors such as the present collection is to bring different perspectives to the several forces at work.

My contribution differs from the others in several respects. One is obvious. Several of us note the impact of some aspect of social science on some aspect of physical science. Porter shows how Maxwell learned from Quetelet. That is a transfer of the *content* of knowledge, if only by analogy. I do not describe such an event. I am concerned with the transfer of a *form* of knowledge. In the social sciences there arise autonomous statistical laws in which chance is no longer problematic. We achieve a new conception of laws of nature, and that is transferred, later, to the physical sciences.

Secondly, I describe a network of connections between the new medicine of the nineteenth century, and the birth and growth of numerical sociology. Taken one by one, the nodes in this network seem fortuitous and irrelevant. Collectively they determine much of the form of sociology, and the conception of laws like those of which Durkheim speaks in my epigraph. I may mention nodes such as: suicide as madness, madness as disease, disease as produced by a structure of causes, the causes being taken as the independent causes for a Gaussian distribution, medical pathology becoming social pathology. Evidently the story is complex but it is a rich one. My account has, I believe, only one predecessor, Georges Canguilhem's *The Normal and the Pathological* – but who needs a more worthy predecessor than that?[2]

Finally, to a greater extent than my colleagues, I emphasize that the events I describe occur within a matrix of disciplinary competition. Who owns suicide? Statistics began as an instrument of state power, and it has never ceased to be an instrument of power, not in the grand sense of governments, but in the more real sense of the power exercised by the helping professionals whose enterprise is information and control. I also note that although the battles were all about knowledge, virtually every claim to know, during the entire development of numerical statistics was based upon error. I believe that things have not changed much since then, but I have tried not to let that prejudice bias the following report.

0. SUMMARY

The literature of suicide has become indefinitely large, but it is remarkably uniform. Hence a paper on suicide and statistics is expected to unravel in one of several standard ways, embellishing this or that doctrine or conjecture about why people commit suicide, or how to stop them. As my paper is contrary to all such writing, a listing of contents may help declare the plan of attack.

(a) Nineteenth-century statistical sociology has as its masterpiece Durkheim's book, *Le Suicide*. The choice of topic was no accident. The statistical study of suicide and other forms of deviancy was a peculiarly French obsession.

(b) The doctrine of suicide as a measure of group pathology was not new with Durkheim, but was present from 1815 on. International debates about who had the "worst" suicide rates began with salvos fired then. The cannoneers were medical men. The French ones were pathologists, concerned with the distinction between normal and pathological organs. Although French medical discourse finally rejected the connection between organic defects and suicide, it retained the concept of pathology – mental or social pathology.

(c) The debate among doctors about suicide rates was transformed by the most important conceptual event in the history of bureaucratic statistics, the publication of official data by the city of Paris and the department of the Seine. It began a veritable avalanche of printed numbers.

(d) After a few years these annual data (and their successors on a national scale) suggested that there were strong regularities about nearly all forms of deviance. These were (it was urged) most beautifully exemplified by "laws" of suicide in terms of statistical facts about distributions by age, time of year, region, method, sex. There arose the notion that there are statistical laws of human and social nature. Hitherto only birth and death had been deemed lawlike.

(e) The fetishism for counting deviants was associated with the French problem of the nineteenth century, the supposedly declining birth rate, attributed to moral degeneracy. Durkheim's first essay on suicide merely continued this old tradition. It asserted a correlation between fecundity and suicide: the lower the first, the higher the second.[3]

(f) The data were fixed upon by two classes of writers. The least likely to be noticed, but perhaps the most important, was the class of physicians engaged in the new enterprise of public hygiene and legal medicine. It was not foreordained that suicide would be a matter for physicians. That was an artifact of the medical imperialism of the early nineteenth century. It is one by-product of the creation of madness as a medical subdiscipline.

(g) The other class of writers was that of the number-fetishists, motivated by both philanthropy and positivism. Although conceptually less important than the doctors, these deviant-counters were in practice essential, for in tandem with the statistical bureaucracies that they fostered, the increasing volume of data about deviance convinced French minds of the existence of endless laws of human, i.e., French degeneracy. These laws were regarded as holding of a necessity akin to that experienced in celestial mechanics.

(h) But physicians were essential because it was part of their discourse that all disease must be fitted into a structure of causes – predisposing, occasional, and so forth. This matters, because there was a problem: how *could* there be such a thing as a statistical law? To notice the regularities is one thing, but why see them as law?

(i) The solution was provided by an unusual blend of the two most ancient sciences, astronomy and medicine. Astronomy provided the Gaussian law of error, significantly renamed the *normal* law shortly before Durkheim wrote. The Gaussian law was the mathematical limit of a binomial distribution. In error theory, it was understood as the consequence of a very large number of little, stochastically independent, causes. That made sense for observational error. It made no sense for social science: unless medicine provided its battery of causes. Medicine did.

(j) By mid-century, there were a great many (French) laws of society and its pathologies, which were understood in terms of a mathematical model relying on innumerable underlying deterministic causes. This created a new problem, called statistical determinism. Free will was challenged.

(k) Shortly after 1860 the free will problem lost interest for French writers. Statistical law became autonomous, not requiring reduction to underlying deterministic causes. This was helped by a revival of emergentist philosophy, in which the whole is capable of being more than the parts.

(l) At exactly this time, German writers undertook vigorous assaults on statistical determinism. Their solution was entirely different from the French one. They had always been holists, but denied, on empirical grounds, that there *are* any statistical laws of society. Hence no problem of free will; also, no autonomy of statistical law.

(m) French writers contributed nothing to the statistical theory of goodness of fit. The issue arose in Germany (well before it arose in London). No accident: the German thinkers needed dispersion theory in order to refute the claim that there are statistical laws.

(n) The turn-of-the-century heroes of sociology are Durkheim and Weber, Durkheim the leader of statistical sociology and Weber the leader of the opposite persuasion. No accident that one figure was French, one German! The "Durkheim" I refer to here goes up to 1900. Subsequently, as is well known, his preoccupations became anthropological.

(o) Durkheim participated in the discourse that created statistical phenomena as something not only *lawlike* but also autonomous. He is an exceptional illustration (because he is so central to his own science) of a transformation that was then occurring in the whole of Western thought. "Western" here is used strictly in European terms; roughly, West of the Rhine.

(p) The transformation, so important to our present scheme of ideas, was largely founded upon error. There were virtually no laws of suicide or of any other kind of deviancy. There was no mathematical reduction to underlying causes, in terms of any theorems then demonstrated by valid arguments. Our entry into a universe of chance – that is, the making possible of the thought that we are in a world governed by stern probabilistic laws – needed mistakes.

1. "SOCIOLOGY IS AN ESSENTIALLY FRENCH SCIENCE"

Or so Durkheim said.[4] More exactly we should say that the chief contributions to nineteenth-century statistical sociology were made in French. There is an irony here, for there is another and quite different sense in which the origins of sociology are French. Auguste Comte made up the very word "sociology". He did so because his own preferred nomenclature, "social physics" or even "social mechanics", was lifted by the great propagandist of statistics, Adolphe Quetelet. Comte was miffed, because he thought that probability and statistics had only incidental connections with the study of society. Hence he coined the term, sociology. Comte, in many ways a born loser, lost again. Sociology became, in France, statistical, a fact commemorated by our recollections of Durkheim.[5]

In the French context we should also emphasize that being numerical does not imply being statistical. In few places except a table of logarithms are more numerals to be found per printed characters, than in the work of Frédéric Le Play on the household budgets of the European worker.[6] Le Play was as antagonistic to statistics as the better known Comte and Claude Bernard. He thought we should never average, but find the very type of the Sheffield cutler or the nomad of the Steppes, and faithfully record just how much the family spent each year on candles or cabbages, on sacks or shoes. Preference for statistical law does not cut across the conventional political divisions. Both Durkheim and Le Play thought that the world was getting worse, and longed for the good old days.

2. SUICIDE IS FRENCH

There is an immense French literature on suicide and society following the end of the Napoleonic wars. My footnotes in what follows will be ample, and yet will only skim the cream off vats of intolerably boring material that is not to be found in any other language. It is no surprise that Durkheim most commonly cites French authors. It will come as more of a surprise that until shortly before he wrote, there was not much else that he could cite. I believe that before the French Revolution the German language wins the suicide writing competition. After that, and especially in the field of monographs, the French win all the way, right up to 1888, when Durkheim published his first major piece on suicide statistics. The only competitors in the home stretch, after 1870, are Italian.

Throughout the present essay, the relevant contrast class is German. As a simple test of what I have just said, we may go to two complementary bibliographies of suicide, both prepared by German authorities. One is a superb example of the bureaucratic efficiency that characterized the Prussian statistical office. It is some eighty pages long, published in 1871.[7] It is discreetly signed "Dr. C. H.," whose opening sentence asserts that nowhere in the kingdom has there ever before been published a systematic survey of the statistical facts of suicide. (There is a footnote recording that this is strictly false. Newly acquired Schleswig-Holstein has been doing that for some time, under Danish provenance.) C. H. begins with a forty-page history of comparative suicide statistics, noting what has been done elsewhere. The superbly informative footnotes direct one to much previous writing. There are two classes, one, official statistics of various nations and provinces, and the other, made up of speculation about and discussion of the statistics. By my count 87 percent of the items in the latter class are written in French. There is no sign that Dr. C. H., who prepared his work during the Franco-Prussian war, was a Francophile. He was a cautious professional, doing his best by his trade.

My second bibliography was published in 1927 by Hans Rost.[8] His main literary productions from 1905 were about catholics and protestants in Germany, but he was also influenced by Durkheim, publishing a "Suicide as a statistical phenomenon" in 1905. Whereas I have no reason to suppose that any other of my authors on suicide were particularly morbid, we note that Rost begins his 3771 items of bibliography with his personal book plate, "ex libris" a death's head. The book ends with a similar bookplate of Max von Boehn. There are ample illustrations throughout the book of people doing themselves in. Mass suicides, harikari, Dante and Vergil looking at the suicides; they are all there in charming monochrome. I confess to having quite liked "The Death of Cleopatra by Guido Canlassi Cagnacci", a rather fleshy picture of undressed Cleo surrounded by many more breasts, with an asp eating her arm. Then I saw the source, a book referred to as *Jena, Diederichs: Das weibliche Schönheitsideal in der Malerei*. Most readers would prefer to spend the night with Count Dracula rather than Dr. Rost, but he is a superb witness to call. If we consider his items from my period, 1815–1888, they tell a tale. I have been subjective in eliminating items that are in my opinion ignorant of numbers, but I come up with 62 percent of the remaining items being in French. The entry up to 1870 would tally with that of C. H. The lower figure for Rost results from the influx of Italian studies after 1870. This is an instructive coincidence, for by and large the two authors are sampling different bodies of work.

The preponderance of French is not a result of a German desire to prove that French are excessively concerned with suicide. But, it will be asked, is there not a simple reason for French concern? Is it not that the French have a suicide problem while Germans do not? On the contrary. Prussia and France had almost identical suicide rates. This is one of the rare constancies of suicide statistics. For large parts of the two populations, French and German suicide rates run hand in hand, despite geographical changes. Thus, as Emmanuel Todd observes in a remarkable neo-Durkheimian study, France and the *Bundesrepublik* have similar enough rates for a number of similar parameters to make one think that there is a "Franco-German" rate.[9] Another constancy is that England and Wales are about a third as suicidal as the Franco-Germans.

I say *parts* of the two populations. The German peoples were not uniform. In the late nineteenth century, Saxony was nearly three times as suicidal as France, matched, in Europe, only by Denmark. I here refer to figures for 1885.[10] Inside today's Germany we cannot compare them to Todd's ratios, because Prussia and Saxony are in the D.D.R., which does not publish suicide rates. But outside of Germany – England and Wales, Scotland, Denmark, France – the ratios of 1885 are about the same as Todd's for 1970. Durkheim was on to *something* when he took suicide rates as stable characteristics of nations and regions.

Saxony, the suicide champion, was the kingdom just south of traditional Prussia. It was no backwater. It had the most dense population of any German state, and the most rapidly increasing population. It was the center of many of the new high-tech industries, and could claim to have the most advanced agriculture in the world. Saxon statistical offices had been preparing excellent reports, but nobody seems to have cared much about suicide. There was no suicide scare. When in 1896 the great socialist journal *Die Neue Zeit* expressed alarm at devastation caused by suicide in Saxony, it did not draw on official statistics published by the Saxon bureaucracy.[11] Instead it relied on recently published French works on comparative statistics. That is typical of the times; and note the year, 1896. That was the year before Durkheim published *Suicide*.

Why were the French so concerned when they were far from the most suicidal people of Europe? From let us say 1815 there was a curious preoccupation with the statistics of moral deviance: crime against persons, crime against property, conviction rates, recidivism, divorce, lunacy, vagrancy, prostitution, suicide. These were all connected with an idea of moral degeneracy.[12] They were constantly before the national mind. This obsession had two elements. One was the fear that a great crime wave was going on: you could not go out in the streets without getting mugged. Every prosperous home subscribed to weekly police gazettes, which in turn prompted the bureaucracies, especially the Justice Ministry, to produce more data.[13]

A closely connected French concern was a declining birth rate. The British and the Germans were exporting great waves of settlers to the New Worlds, and yet their home populations still increased. France sent almost no emigrants, yet its population was thought to be declining. This was a result of "moral degeneracy", which became the focus of a century of discussion.

Durkheim was eminently part of this web of interconnecting ideas or prejudices. Thus the very passage that I quote at the head of this paper contains a footnote, and refers to a long footnote on the preceding page, which discusses crime rates and marriage rates: "In truth, here as in the case of suicide, statistical figures express not the mean intensity of individual dispositions but that of the collective impulse . . . " Then, in the next footnote, attached to the point at which Durkheim has said that suicide statistics show there are social forces that dominate individual consciousness, we read, "[h]owever, such statistics are not the only ones to do so. All the facts of moral statistics imply this conclusion, as the preceding note suggests."

If there were data about so many deviancies, why then did Durkheim choose suicide for his monumental study? We need not postulate some dark and autodestructive part of Durkheim's soul, although doubtless, like so many of us, he had one of those. The choice of suicide is, I think, overdetermined. First, there is the outright statement of his first suicide paper of 1888. Suicide is in some straightforward sense the contrary of procreation. The national problem was low population, said to be caused by degeneracy. What better measure of this kind of degeneracy than suicide?

Second, as I shall show in the next section, the suicide rate was the first deviancy rate to be used in comparisons of "the quality of life".

Other comparisons were made in due course, crime being the most notable. Indeed in the work of Guerry to be described below, crime and suicide were two parallel obsessions in national comparison. Yet Guerry, as we shall see, spent his own life collecting suicide data. For crime he went to the Justice Ministry.

Third, one would not use crime as a measure of the quality of social life in the nation as a whole, because crime was the province of the criminal classes and *les misérables*. Suicide runs across all social classes and so is a measure of the national condition. Fourth, other vices were measured for sure: prostitution for example.[14] But no one with a sense of history would connect concourse with prostitutes and anomie! Madness and various kinds of mental alienation will not do, for they may have a purely organic origin. Finally, there is the obvious, naive, uninteresting (and probably false) intuition that as suicide is an individual decision to leave humanity, it is a measure of the number who felt "pushed over the edge" by their alienation from society. In short, suicide and Durkheim were made for each other.

3. ESQUIROL V. BURROWS

Within the standard Western *mores* we think that a high relative suicide rate is a sign of something bad about a culture or group. It is characteristic of most sloppy thinking about suicide – I here refer *not* to Durkheim but to our common attitudes – that a moment's reflection would convince us that the suicide rate is at best an indicator of something-or-other. It is common "knowledge" that Sweden is the most suicidal part of Western Europe. It is a fact that Eire is the least suicidal. Whatever the charm of Dublin and Cork, many of us might as well jump to the conclusion that a *low* suicide rate is what's bad.

We find it "natural" to compare suicide rates of different groups. My story of French suicide statistics begins with just such a comparison. Folklore of the eighteenth century had it that the English were the maddest and the most suicidal people in Europe.[15] Madness was "the English malady" and suicide was *melancolia anglica*. So much was readily understood in terms of the gloomy English climate and the predilection of the English for science.

In 1815 this was put in question. Dr. George Burrows had been reading the London Bills of Mortality, and the reports of Parisian mortality in the 1813 *Journal de Médecine*. He had his own journal, *The London*

Medical Repository, in which he wrote up his findings.[16] His chief aim was to encourage British official statistics to pull up its socks, because the French data were done so much better. He also mentioned, however, that in 1813 Paris had 243 drownings to 101 in London, and 141 "dry" suicides to London's 35. He correctly noted in a later discussion that it was widely understood that most drownings in the Seine were suicidal. Burrows cautiously wondered whether the greater suicide rate of Paris could be attributed to "the late political events" which had "annihilated religion". He thus unwittingly set the stage for international comparisons of suicide statistics, and firmly imprinted the idea that suicide was a social indicator.

French readers had two options. They could have accepted the idea, and denied the facts, or they could have accepted the facts and denied the idea. Either way, national honor would be saved; Parisians need not be held to live worse than Londoners. Unhesitatingly, the former option was preferred. The English facts were wrong: the idea was right. The canonical statement of this opinion is in the article on suicide in the 60-volume French medical encyclopedia. The author was J.-E.-D. Esquirol, second only to Pinel in the early history of French psychiatry.[17] Since 1811 he had been physician at the Salpêtrière (Pinel's hospital for the mad, which, with Bicêtre and Charenton, were the great asylums of Paris and, arguably, of the world). In 1823 he was to become inspector-general of the medicine faculty of the University of Paris; in 1826 he became chief physician of Charenton. Throughout the nation asylums were built according to his designs – Rouen, Nantes, Montpellier. This was a man of weight.

His essay on suicide became classic, and with small modifications was recycled into various of the textbooks that Esquirol published. His student J.-T. Falret published the official Esquirol line in his 1822 dissertation,[18] the year after Esquirol's dictionary essay on suicide. Esquirol and Falret denounced the doctrine of Burrows, discounting his alleged facts and remarking that everyone knows the English are more suicidal and certainly more mad. Burrow's own response was stately. As soon as he had read Falret's book, he printed an anonymous review in his *Repository* of Falret's "excellent, even classical" study. Then he signed a "Reply to Messrs. Esquirol and Falret".[19]

The debate continued for some time, but we may spare the details. What matters to our understanding of "Durkheim" is that the comparison of national suicide rates, with their implications for moral science, was established right at the beginning, and fed the French suicide obsession throughout the century.

Esquirol's essay was not primarily an assault on Burrows, although Burrows did loom surprisingly large. The essay was chiefly a play for power. Who owns suicide? The moralist, the confessor, the lawyer? No: henceforth the suicide shall be the property of the medical man. Esquirol, in his official position as physician at an asylum, might have been expected to care for the bodies of the deranged. A generation earlier that had been the role of his predecessors. But at the time of Pinel there had been a remarkable takeover. The mad were to be treated by doctors. No longer were they to be put away and restrained. The transition was not peculiar to France. Burrows addressed his 1820 *Inquiry into Certain Errors relative to Insanity and their consequences, physical, moral and civil* to John, Lord Eldon, General Guardian of Lunatics. The title of Guardian is not symbolic but descriptive of the world of the asylums before 1800 or so. The Crown provided a Guardian for Lunatics, who were put away. With the invention of what we would now call psychiatric treatment, that power was wrested from the Guardian, and became the property of the physician. Suicide was only incidental to that mutation, but the connection with madness and medicine matters to us here. Esquirol was able to claim control over suicide on the ground that all suicides were insane, hence medical property. "I believe that I have demonstrated," wrote Esquirol, "that a man does not attempt to end his days except in delirium, and that suicides are mad."

As we shall see in section 6 below, this was a much debated topic for the entire period 1820–1848. Some polemics were mild: at least the noble suicide, typified by Cato of Utica, was not insane. Some of the arguments were, by our standards, bizarre. It is to be remembered that around 1800 medicine had changed from a doctrine that disease is imbalance in the whole person, to the idea that every disease has its own peculiar organ. There followed a pair of syllogisms. Suicide is madness. Madness is a disease. Therefore every suicide is diseased. But each disease has its own organic seat, its organ that causes the disease. Therefore there must be an organ associated with suicide. Even in 1840 the question of organs was being seriously posed:

What organ? [creates suicidal tendencies] The organ that presides over the intellectual and affective faculties. . . . It is necessary to locate the predisposition [to suicide] or organic modification. It exists in those individuals who, without plausible motives, for a slight or imaginary cause, experience a disgust with life and an irresistible propensity towards suicide.[20]

The doctrine of defective organs is the root idea of pathology. Hence from the start of the medical conquest of suicide, suicide was taken to be a pathology. Even when the exceptional medical views of the first half of the nineteenth century were fading away, the concept of suicide as pathological remained. Durkheim inherited it as social pathology.

Thus from the start of the Burrows-Esquirol debate suicide had been used as a means of comparing national and group character. Simultaneously suicide, through its being absorbed into medicine, entered the discourse of pathology. By the end of the century suicide had become the paradigm indicator of group pathology.

4. A NEW KIND OF NUMBER

The Anglo-French debate about national suicide rates was soon to be settled not by physicians but by bureaucrats. One of the half-dozen most important events in the history of statistics is the inauguration of the annual series: *Statistical Investigations into the City of Paris and the Department of the Seine*. The national ministries soon took over the work of the capital, and annual volumes of statistics came out of Education, Justice and the like. The importance of the Paris volume lies in ideas, not their specific numbers. First, the chief organizer was the aging but immensely able physicist, Joseph Fourier. As well as being the responsible administrator, he wrote the unsigned methodological introductions to the volumes. His method was taken up by others. Adolphe Quetelet's small textbook of 1828 is very similar to what he read in Fourier in 1826.[21]

Fourier is important for methodology, but his volumes are important for what is counted. Births and deaths are there, excellently done, but the dominant topic was deviance. The hospitals were studied, but the great asylums of Bicêtre and the Salpêtrière (the former for men, the latter for women, the latter in which Esquirol was so prominent a figure before he moved to Charenton) received vast fold-out pages. There were numbers for admissions and releases, and for deaths in the establishment. There were numbers for length of stay, but here we should attend especially to the causes of the madness of those incarcerated. There were some 19 physical causes, such as congenital idiocy, drunkenness, deformations of the skull, masturbation, pregnancy, libertine behavior, paralysis and so forth. The mental causes included exaggerated religion, ambition, political events, rage, love, and simulated madness.

Suicides were catalogued along the lines of madness, but there were important differences. A decisive conceptual step was made in passing from the suicide table of 1821 to 1822. In the volume for 1821 suicides were classified according to *motives*. In the tables for 1822 they were classified according to *causes*. It happens that the motives and the causes are the same – but what was seen as a motive (and the topic for the moralist) was turned into a cause (and so the topic for the physician). The motives of 1821 – the causes of 1822 – were love, various kinds of malady such as disgust for life, weakness and alienation of the spirit, quarrels and domestic problems; then there were debauchery and other forms of misconduct, indulgence, fear of punishment.

As soon as this bureaucratic organization of causes was put together with the medical theory of causation then current, we obtain an importantly expanded scheme. I shall put great weight on that later on. There were other routine items of classification, age, sex, and married state. Then there was an essential ingredient of any kind of medical statistics of the day, namely the season at which the event took place. No medical statistics lacked meteorology because the air and its condition were thought to interact with people and their medical condition. Yet suicide had its own unique and inevitable classification: the *method* of suicide. The possibility of cross-correlating methods (which were objectively determined) with other determinants created, as we shall see, a field day for discovering a hitherto undreamt of domain of statistical regularities, soon to be elevated to the station of "laws" of moral science.

The Parisian series edited by Fourier was, as I have said, in large part superseded by national statistical annuals. Thus for example one major entry in the *Statistique generale de la France: Territoire et population* of 1837 tabulated accidental deaths and suicides for 1826–1836. The Ministry of Justice, which had been publishing data about crimes, prosecutions and convictions after 1826, assumed suicide under its wing on 29 July 1836. This may seem a slap in the face for the medical men, if, as I claim, they have been hankering to own suicide. Not at all. They helped create the new branch of their profession, legal medicine, epitomized by that rich source of material of the day, the *Journal de l'hygiène publique et de médecine légale*, founded in 1829. I shall say little more about the official statistics of suicide after 1837. Important twentieth-century work made ample use of these later nineteenth-century data; one thinks particularly of the man often called Durkheim's successor, Maurice Halbwachs.

5. THE ENTHUSIASTS AND THEIR STATISTICAL LAWS

The greatest of French amateur statisticians of deviance was A.M. Guerry. A lawyer of private means, he was well connected with officialdom, for his cousin Guerry de Champneuf helped establish the statistical work of the Justice Ministry. But it was the amateur who is the more memorable in the annals of statistics. His first major work won the Prix Montyon for Statistics in 1832.[22] The Montyon prizes were a pre-revolutionary legacy, set down between 1780 and 1787 by the philanthropist Baron de Montyon. Guerry's book was *An Essay on the Moral Statistics of France*. It is in itself a superb object of noble dimensions and with fine maps indicating the geographical distribution of crime. It was a statistical first in several ways. For example it used what we would now call rank-order statistics to refute the proposition that education helps diminish the crime rate. On the contrary, the higher the level of education, the higher the level of crime. That is another story. The next topic of moral statistics after crime was suicide. "Among the subjects encompassed by moral statistics, suicide is (as Guerry observes) one of those that has attracted the most lively attention, and that has been most discussed."

Guerry was annoyed that outside of Paris there was no attention to why people kill themselves, nor were even the easiest facts, those of sex and age, recorded. He propounded a schedule in which every constable should record, on the spot where the corpse is found, the sex, age and state of health. Profession or social class. Residence, birthplace, marital state, number of children. Finance: rich, comfortable, poor or miserable. Education: literate, can read and write, illiterate. State of mind. Morality (judicially condemned? adulterer? gambler? prostitute? concubine? drunkard?). Religion.

Then there should be a record of the place, the medical circumstances, the data and hour, and the weather. How was it done? Why was it done? Was a letter left? Previous attempts? Was there a parental history of madness or of suicide? *Objets trouvés* (at the scene of the suicide, in the victim's pockets, and so forth).

Guerry's schedules were adopted only partially and regionally. He himself outdid a phalanx of constables and clerks. "He obtained from the police archives 85,364 individual records of suicides committed between 1836 and 1860 . . . "[23] In yet another concern, he categorized 21,322 people accused of murder and then analyzed them into 4478 groups of individual motives, from which there emerged 97 classes of principal motives. An *éloge* relates that the numerals in this analysis written down one after the other would stretch 1160 meters. Guerry was a fetishist of counting; his biographer becomes a meta-fetishist. Guerry is a personal testament to the avalanche of printed numbers that occurs 1815–1848.

Guerry's work turned, after 1832, to comparative statistics of crime and of suicide. His comparison, in the tradition of Burrows and Esquirol, was between England and France. He was much praised, although seldom emulated, in England. Thus his materials had a special display at the 21st meeting of the British Association for the Advancement of Science in 1851 (that, it will be recalled, was the year of the Great Exhibition). When his last book was published in 1864, it was given much praise at the Statistical Society in London.[24] This work was shown at the B.A.A.S., in 1865, with none other than William Farr giving a laudatory speech. Nor was it only the statisticians who were moved to admire. It is reported (by Diard, n. 23) that Countess Flavigny, author of *L'Enfance chrétienne* and other improving works, "never opened this work without a feeling of respect." (Me too.) Guerry stated the point of his masterpiece thus:

> What is the use of moral analysis? It is, above all, as in the physical sciences, to show the connections between phenomena, to give knowledge of intellectual realities considered in themselves outside of any idea of practical application. In the full rigorous use of the terms, science consists of knowledge, and not in deciding what to do.

This is positive science, distinguishing fact and value. "In stating rigorously the numerical facts bearing on society, moral analysis forms the experimental basis of the philosophy of legislation."

What are the intellectual realities considered in themselves? They are the regularities that leap to the eye from the systematic collection of numbers of deviancy. Suicide and crime are offered as the most favorable examples of such regularities. Guerry saw this right from the beginning, when he could rely on only four years of records.

> During these four years, the proportional number of suicides committed in each region

did not vary by more than 3% about the mean. In the central region of Paris and the Seine it did not vary by more than 1%.

More fascinating than the absolute suicide rate are the regularities among cross-classifications. It is here that we find the origins of the idea of correlation and regression, neither of which would have been thinkable, in our ways, without that avalanche of data upon which Guerry was to rely, and to which he was to contribute.

The systematic ratio between male and female suicides, the regularities by seasons, the comparisons by region: all, supposedly, are stable. The seasons are instructive. Before there were any numbers, there was a faith in suicide being *melancolia anglica*. One explanation was that gloom means doom: the worse the weather, the more frequent the suicides. But when statistics became available, such notions were doubly refuted. The English (whatever their past and unsubstantiated proclivities) were the least suicidal people of prosperous parts of Europe. Moreover it turned out that midwinter was the least likely time for suicide. There appeared to be an almost sinusoidal curve of suicide that peaked in July and was at a minimum in January. This was rather constant throughout the temperate zones of Europe.

Regularities in methods of suicide were even more striking. Burrows had correctly noted that Parisians preferred drowning; then (for a time) firearms were preferred, followed by carbon monoxide poisoning. This last became more popular as more and more of the tenements began to use charcoal braziers indoors. The English, on the other hand, shot and hung themselves.

Guerry's regularities were no mere curiosities. As soon as his book had appeared in 1834 the future diplomat Henry Bulwer published two volumes of observations that gained quick success. He firmly urged that every race has its own character – a familiar theme, but in the new era of counting and of measurement, he could defend this on the basis of numbers. There are statistical differences between the races. "I am led to these reflections by a new statistical work by M. Guerry, a work remarkable on many accounts . . .[25] He thought that Guerry's tables on the regularities of crime and suicide could "afford sufficient matter for the most important work on history and legislation that has yet appeared." I suppose that Henry Buckle's 1857 work on the history of civilization in England seems to fulfill the prophecy: it, at any rate, is completely motivated by the belief in underlying statistical regularity.[26] Bulwer gives an example of the kind of thing that so captured his imagination:

> Anyone little given to the study of these subjects would hardly imagine that the method by which a person destroys himself is almost as accurately and invariably defined by his age as the seasons by the sun.

Some fifteen years later, in 1848, Dr. E. Lisle won a Prix Montyon for his book *On Suicide*.[27] He stated what had become a commonplace: "All the facts contained in this first part of my book tend to demonstrate this remarkable proposition, already announced by a certain number of writers, that moral facts obey, in their repetition, laws as positive as those that rule the physical world." This talk of positive laws was embedded in the philosophy of the time:

> It is no longer permissible in our days to seek the truth in pure theory, in vain abstractions or gratuitous hypothesis. The rigorous observation of facts has become quite rightly, the starting point and the foundation of our knowledge. From this enlightened positivism is born the application of statistics to medicine and to the study of moral and political questions.

There is double irony here. First, notice how the word "positivism" of the anti-statistical Comte has been snatched away and made part of the statisticians' ideology. Second, for all this talk of facts and patient pre-theoretical observation, the regularities so admired by a Guerry, a Bulwer or a Lisle do not exist.

I have said nothing of the regularity salesman who is best known nowadays, the great publicist of statistics (but also Astronomer-Royal in Brussels), Adolphe Quetelet. His remarks, to the effect that the statistics of crime prove that there is a constant national budget of the scaffold, are often quoted. There are several reasons for passing over his work here. First, suicide was not a prime concern of his. Second, his contributions to statistics and sociology have been meticulously described long ago by Joseph Lottin (n. 6) and are discussed from an up-to-date perspective by Porter (n. 26). Quetelet was an immensely important public figure, founder of journals, societies and the international statistical congresses. I do not mean to diminish him. I have wished only to emphasize that he was not alone, but was surrounded by people who did the detailed work that he communicated, and who breathed, and talked, in the network of attitudes that he helped popularize.

6. MEDICAL CAUSES

Few lists will strike the modern eye as more curious than the following one:
- Heredity
- Temperament
- Age
- Sex
- Education
- Reading novels
- Music
- Theatrical performances
- Climate
- Seasons
- Masturbation
- Idleness

That is a list of predisposing causes of suicide, to be found in the 1822 book mentioned earlier (n. 18), written by Esquirol's student Falret. Medicine had long inherited a division into four kinds of causation of disease or of death: predisposing causes, direct occasioning causes, indirect causes, and general causes. In the case of suicide, occasioning causes were more numerous than the above predisposing ones. They included passion, love, remorse, domestic problems, dreams of fortune that have been frustrated, pride and humiliation, obsession with gambling dishonor, outrage at lost virtue, waves of passion, jealousy and conjugal tenderness. Indirect causes included alcohol, syphyllis (and mercury, its treatment), opium, physical pain, scurvy and pellagra. General causes were general indeed: governments, civilization, religious belief, sects and public morals.

Recall Esquirol's doctrine that suicide is a kind of insanity, coupled with the pathologist's view that all disease, including madness, has an organic ground. Esquirol dominated the Salpêtrière, but had less influence at the male asylum of Paris, Bicêtre. There F. Leuret pioneered the notion that lunatics can be much helped by moral suasion.[28] The couch looms on the horizon! In his book we are told that "Suicide is not always an instance of madness." It is difficult for us now to recall how live an issue this was, and that Leuret was making a strong statement. He published in 1848. In 1845 C.-E. Bourdin had opened *his* tract with the words, "Suicide is a monomania."[29] When one attends to the "real causes" of suicide, Bourdin continued, one sees that one is confronted by a "veritable pathology." This in turn was in part a riposte to G.-F. Etoc Demazy's book of the year before. It held suicide to be voluntary, typically rational, a consequence of real circumstances, and not from "aberrations similar to those that characterize madness."[30]

Dr. Lisle, the avowed positivist and advocate of statistical regularity cited above (n. 28), was part of the anti-madness, anti-Esquirol school. He quoted Leuret on his title page, including the sentence "Suicide is not always an instance of madness," and, "in a number of cases suicide is a result of madness, but in other cases it may be thought of as consequent upon causes that provoke and accompany the act, much as we think of a weakness, a fault or a crime." By mid-book – I take it to have been printed in chunks – Lisle entered a frantic footnote: we should delete the word "always" from Leuret and write simply, "Suicide is not an instance of madness."

Although these doctors were not fans of Esquirol, they were doctors: the medical takeover of suicide had succeeded. They were the ones authorized to speak about suicide and its statistics. Since they were medical men, they had the medical structure of causation. By the time of Lisle the list of predisposing causes had swollen vastly beyond Falret's. The extended lists included entries which are there precisely because (for Lisle) suicide is *not* insanity: regret at having disposed of all or a part of one's fortune, frustrated hopes of a gift or inheritance, disgust at one's social position. What business is it of the doctor, to write, in his role of physician, on the problems of someone who regrets having given away his fortune? Lear is surely for the dramatist and the moralist. No: we are witnessing the way in which the medical profession usurped ethics and the theater.

We noticed that between the Paris data for 1821 and those for 1822 the tables for suicide were changed from "motives" to "causes." Lear's problem would be a motive, not a cause, of suicide. But once suicide had been adopted by medicine, it became a cause. The works on suicide to which I refer are primarily statistical studies, prepared by medical men. Suicide statistics were embedded in talk of causes, and the causes proliferated endlessly. Moreover in the medical structure, an event was not the product of one cause, but of four causes of different kinds. The collective data of suicides were then the result of a large number of different causes, each acting on this or that individual, but collectively producing the allegedly stable statistics of suicide. That leads to the next step in my story: an odd combination of the two oldest sciences, medicine and astronomy.

7. THE BINOMIAL MODEL

Let it be that there are great regularities in moral statistics, that is, the statistics of deviancy. Let it appear that this stability is almost as great as is found in the planetary motions. How is that possible? The crude answer was that it is a consequence of "the law of large numbers." I shall not trace this notion, and its misunderstandings, here. Suffice to say that Bernoulli's result, published in 1713, was well known. If there is a chance event E with probability p on an individual trial on a chance set-up, then, if many stochastically independent trials are made, the average number of trials with outcome E will be about p. This theorem may be taken to explain, or at least clarify conceptually, a certain kind of statistical stability.

S.-D. Poisson was much taken by the apparent stability in conviction rates in French courts, which became available after 1826. But this stability could not be understood in terms of Bernoulli's result, for the chance of conviction must vary from trial to trial, depending upon the quality of the evidence. In 1835 Poisson introduced the expression "law of large numbers."[31] It came to be used in two senses. First, it seemed to denote an empirical fact about long trials, e.g., conviction rates are constant. Secondly, it was used by a few who understood the matter to denote a new result proven by Poisson. If the probability of E from trial to trial varies, but is governed by some overall probability law, and if the average probability of E is p^*, then in many trials the proportion of E will be about p^*. Poisson took this to explain, or at least to help us understand, the kind of stability found in French jury trials.

Very few people in France understood this result. I.J. Bienaymé was among the best placed to do so. He waited until his mentor was dead, and then announced that there simply was no new theorem. Poisson had just made a mistake in thinking he had proved something different from the old result of Bernoulli, or so Bienaymé said.[32] Nevertheless the expression "law of large numbers" became standard among French statisticians, medical writers, and philosophers. It is useful to see how it was understood. In brief, stability was thought somehow to result from the interaction of a large number of minute independent causes, which collectively would yield the regularity promised by the law of large numbers.

At this point astronomy entered. The theory of errors was by no means the work of one man, but among its founders C.F. Gauss is paramount. Early in the eighteenth century it was known that a binomial distribution (the distribution of k successes in n trials) becomes in the limit our familiar bell-shaped curve. This fact became of major importance when Gauss used it to model errors of astronomical observation. The idea was that each observation was caused by the true position of the object seen, but was affected by a large number of small errors attributable to the instruments, the observers, and the like. It was thought that these errors did not themselves interact causally and so could be modeled by stochastic independence. Gauss brilliantly applied his analysis to observation data, and used it to derive most probable orbits.

Gauss's result could be understood in terms of a large number of independent causes, collectively conspiring in a mathematically necessary way to produce the Gaussian law of error. There is a detailed history of the working out of the mathematics, even though much of it, after Gauss, had more handwaving than proof. That does not concern us here. We turn to another piece of handwaving.

Quetelet's great contribution to the conceptual development of statistics lay in his remarkable assertion that the vast majority of social and biological phenomena are distributed in the same way as the Gaussian curve. His doctrine became fixed in English when Karl Pearson (in 1893) chose to call the Gaussian curve the Normal law (with the implication that other distributions may be pathological). Quetelet's rhetoric began with the casual observation of summaries of chest diameters of Scottish soldiers, which from time to time had been published in the Edinburgh medical journal. Highland chests are distributed just like the Gaussian curve of errors. It is just as if one soldier of average girth had his chest measured by an incompetent tailor 7000 times. In the present century our first sceptical reaction to Quetelet would be to apply some test of goodness of fit, to discover whether in fact the published data did reasonably fit a Normal distribution. In Quetelet's time there were no such standard tests. That was just as well, for only by a very great stretch of the imagination can the data from Edinburgh be said to follow the Gaussian curve of error.

No matter: here is the beginning of an understanding of the stability of moral statistics. But where are the little independent causes that are supposed to underlie the Gaussian distribution and, in general, laws of large numbers? Our fictitious tailor may make his errors due to different causes at different moments, but we still have the image of error. How is that to be transferred to moral statistics?

My answer is plain. What moral statistics needed were little independent causes. Fortunately moral statistics lay in medical discourse, which provided, page after page, the classifications of predisposing causes and the rest. Doctors served up the causes in terms of which one may understand the stability of some moral statistics – in terms of the folklore of what makes laws of large numbers possible.

I do not claim that there is anything coherent in this, anything rational, anything sound. It is important, for example, to my thesis that Poisson's theorem was not understood in the following two generations, at least in France. Had it been, this nonsense would have been substantially undercut. I assert only that this cant made intelligible the radically new notion of statistical law in moral affairs. One gets some grip on how new it was by the problem of statistical determinism.

8. STATISTICAL DETERMINISM

With the advent of the quantum theory it became fashionable to argue that indeterminism in physics made a place for free will in human action. I have heard this aptly compared to fitting square pegs into round holes. But at any rate the idea that laws of nature should be statistical was taken to make free will more intelligible. The situation was entirely different in 1850. Let it be supposed that there are laws of suicides when sufficiently large numbers are involved. It is asserted, even, that the distribution of suicides among the arrondissements of Paris was uniform and constant. Next year's results can be foreseen now by the statistician. But successful suicide is the most mortal of sins. One can repent after every other mortal sin has been committed, but a truly successful and instantaneous suicide leaves no time for that. Hence the worst possible sin is governed by deterministic law. Worse, let it be ordained that 20 people will kill themselves in the 8th arrondissement next year, give or take one victim. Then it may not be fixed which souls must do away with themselves, but such souls there must be, condemned now to that fate. No amount of free will on the part of the inhabitants can prevent that.

The problem was largely a rehash of one used to bait the phrenologists a couple of generations earlier. They had asserted that every human propensity is located in an organ in the brain, the propensity being the larger (in the simplistic version) according to the corresponding bump on the skull. Since many of the propensities thus charted were virtues and vices, moralists protested. If I have the bump of avarice or lust, then I am not free to be ungrudging or chaste. The phrenologists replied that a tendency or propensity inclined (as Leibniz might have put it) without necessitating.[33]

I must not be taken to be introducing an irrelevant matter here. As is well known to historians of medicine, the phrenologists played an important role in the switch from total-body medicine to the pathologists' organ-directed disease. More specifically, perhaps the first book stating the Esquirol doctrine of madness and suicide was that of E.J. Georget, a dissertation of 3 February 1820, published later in the year.[34] That was the year before Esquirol's articles. Georget was widely regarded as Dr. Gall's most brilliant student – it was a tragedy for the development of phrenology that he died so young (or so it is said in works of the Esquirol school). When the statisticians were confronted by the free will problem in the 1850s, their response was the same as the phrenologists, although they preferred the word "tendency" to "propensity." At most we can say that there is a greater tendency to suicide in the 8th arrondisement as opposed to the 14th, or in France as opposed to England and Wales. No one is compelled to suicide.

Many elements in the debate are well reported in Porter's chapter "Statistical Law and Human Freedom" (n. 26, 151–192). In the case of suicide it was not irrelevant to the suicide-as-monomania debate. Thus Bourdin (n. 29), noting that "the question of suicide brings in the question of freedom," goes on to say that there is no conflict between statistical determinism for suicide, and the religious doctrines of liberty and responsibility: for the suicide is mad and so not responsible. He distinguishes two kinds of fatalism. One is a contrary of freedom. The other is bad luck, the misfortune of "the blind, the idiot" – and the suicide. A man is no more responsible for his suicide than the sightless are responsible for their blindness.

There was more of a problem for those who held that suicide is not a disease or a kind of madness. However, as Porter recounts, the stir about freedom and statistical determinism had died down in France by the late 1860s. The issue was effectively dead when Durkheim wrote. But he was too steeped in the suicide literature to let it go. It is in just this connection that he makes one of his most remarkable statements about the laws of society:

Without wishing to raise a question of metaphysics outside our province, we must note that this theory of statistics does not deny men every sort of freedom. On the contrary, it leaves the question of free will much more untouched than if one made the individual the source of social phenomena. Actually, whatever the causes of the regularity of collective manifestations, they are forced to produce their effects wherever they occur; because otherwise these effects would vary at random, whereas they are uniform. If they are inherent in individuals, they must therefore inevitably determine their possessor. Consequently, on this hypothesis, no way is found to avoid the strictest determinism. But it is not so if the stability of demographic data results from a force external to the individual. Such a force does not determine one individual rather than another. It exacts a definite number of certain kinds of actions, but not that they should be performed by this or that person. It may be granted that some people resist the force and that it has its way with others. Actually, our conception merely adds to physical, chemical, biological and psychological forces, social forces which like these act upon men from without. If the former do not preclude human freedom, the latter need not. The question assumes the same terms for both. When an epidemic center appears, its intensity predetermines the rate of mortality it will cause, but those who will be infected are not designated by this fact. Such is the situation of victims of suicide with reference to suicidogenetic currents (n. 1, p. 325)

9. STATISTICAL DETERMINISM: GERMANY

My contrast class to the French obsession with suicide is German. The contrast comes out again in the concern with statistical determinism which, I have said, had suicide as one of its main roots. As one might expect, there was no enthusiasm for such determinism in Germany, which rejected *Queteletismus*. Porter has reported many of the details, but a few remarks may be useful here.

First, although there was little German interest in suicide by mid-century, there was some early writing on the topic. In 1828 Johann Ferdinand Heyfelder published a rather frightening tract showing the rapid increase of suicide all over Europe.[35] Thus suicide (as reported) in Berlin went up as follows: 1788–1797: 62. 1797–1808: 123. (Break for reorganization after Jena.) 1813–1822: 546! Quetelet reported these results in his journal, saying that "it results from the collection of data gathered by M. Heyfelder, that suicide is continually on the rise" – a proposition which does not mesh very well with the assertion that suicides are constant.[36] It is doubtless relevant that Heyfelder had studied in Paris in 1821 just at the time of the Burrows-Esquirol fracas, and more generally, at the beginning of the craze for enumeration. So he imported yet another French fashion to Berlin. Between 1824 and 1828 he published five statistical studies.

Fascination with suicide lapsed in Germany until the statistical determinism debate took place in France. In France the debate simmered down in 1860 or so, and *pace* Durkheim, there is little evidence that anyone took the problem very seriously thereafter. How different among German writers! There was hardly a treatise of national economy or population statistics that did not take a swing at the problem. But the approach was totally different from the French one.

French writers by and large said that there *are* laws of moral agency which, in the large, are as reliable as the laws of the cosmos. But if they are like cosmological laws, will they not impinge upon free will? The German answer was that there are no such laws. It is not the case that such regularities are exhibited in the moral realm.

There is a good deal at work here. First, most French writers, until 1865 or so, and who dealt with the statistics of morality or medicine, were positivists. They thought of laws as regularities. Hence they did not feel much of a threat to freedom from the fact of regularity. German writers on statistics were seldom positivists. They made a sharp distinction between law and regularity. Only a law could interfere with freedom. A law operated from an underlying principle or cause; not the statistical causes of Gauss but the identifiable and individual causes of Kant. Bureaucrats publishing in official publications took the time to make this philosophical point.[37] One could never call something a law unless a cause were known. There are no statistical laws of suicide.

Moreover, and this is an empirical rather than a philosophical assertion, it was urged that there *are* no significant statistical regularities about moral phenomena. The claims of German writers on this topic were not about differences in data, but about differences in perception of data. They looked more sceptically at data provided in French, and saw no regularities there. It can be argued that this reflects an attitude to probability that is Eastern, holistic and conservative. It contrasts with the Western, atomistic and liberal conception of the behavior of a group.

Third, German writers did not perceive the regularities urged on them by the French, but they felt an obligation to prove that no regularities exist. Thus they devised measures of dispersion, to test the fit between data and theoretical model. Where Quetelet had seen the Gaussian distribution everywhere in the biological and social worlds, Lexis applied new measures, published in 1875, to show that almost no phenomena of that sort are regular.[38] Thus we are concerned here with large issues in the formation of statistical concepts, and also with the nature of sociology itself. We may find the same explanations for the difference between the tradition of Durkheim and that of Weber, as we find for the difference between the so-called continental approach to statistics – primarily Prussian and Russian – and the Western tradition that grew up in France, Britain and then in America. In this paper we are restricted to more limited claims. The point of the German contrast is to show that there was nothing in the phenomena of suicide and other kinds of deviancy that created the tradition of moral statistics. It arose from a peculiar French conjuncture of medicine and bureaucracy, modified by bizarre concatenations such as those connected with the mathematics of the Gaussian curve.

10. EMERGENTISM

The debate about statistical determinism could have been resolved in France the way it was in Germany. The alleged regularities about suicide could have been denied. Why were they not denied? Should we say simply that too much was invested or should we invoke a view of how the numbers themselves were conceived, a view about the number-discourse of France itself? We need not answer here: the fact is that the industry of moral statistics did not take the German turn.

What did happen was that statistical regularities were given a life of their own. They became, in a sense that I have explained in more detail elsewhere, autonomous.[39] On the one hand, it was accepted that there is nothing paradoxical about the idea of a statistical law of human affairs. That was because of the official myth of reducibility to a supposed theorem and the interaction of myriad independent causes. I call it a myth because in France there was no attempt even to understand Poisson's law of large numbers, let alone extend the idea with further mathematics. The law was at first well understood in the Petersburg school. Serious research on how apparently homogeneous stability can arise from heterogeneous events was continued primarily in Berlin in the 1880s. There it was important to have an understanding of the cases when a stability really is founded upon underlying stochastic randomness, and those cases when it is not. French writers seem not to have cared. They accepted the myth and moved to the next stage.

The next stage was to treat seriously the idea of inherent tendencies in groups. The myth said: all the tendencies are explicable in terms of underlying causes. But because it was only myth, no one investigated the question. One took for granted that there could be group tendencies. That is, one could talk legitimately, and without hesitation, of the group, without any reduction to atomistic behavior. Positivists of probability became *de facto* holists while remaining *de jure* atomists. Nothing more than a little philosophy was needed to push them to the final, Durkheimian, step.

I do not wish to imply the absurd proposition that nineteenth-century French intellectual life was predominantly "positivist". At most that could be said for the long tradition of medical numerologists and the like. In philosophy we need only think of the long domination of the school of Victor Cousin, or of Charles Renouvier. Renouvier is called a French neo-Kantian, a near solecism. Cousin is, I think, not integral to our account, but Renouvier is. He wrote amply on what he called the law of large numbers, and on statistical determinism.[40] He did much to create the possibility of a rebirth of emergentism in France. Also, we may add, in America, for it was Renouvier who was first publishing William James. But if we are to single out a single text to set the stage for Durkheim, it is that of Emile Boutroux.[41]

Boutroux submitted the book for his doctoral thesis at the Sorbonne in 1874 and it was published next year. Many years later, for an American edition, he wrote that "The idea I set forth at that time seemed paradoxical and very unlikely to be taken into consideration." He was in the right place at the right time. He taught the old doctrine that there is a hierarchy of kinds of being, each with its own laws that emerge from but are not derived from beings of lower levels of organization. This is familiar, and it had the benefit of being published in an era when Darwin and Spencer were household names. Boutroux did not go beyond psychology to sociology. He resented the way in which blind habit becomes encrusted in human psychology, so that "Statistics makes a legitimate invasion of the ground left abandoned by free will, and its conclusions are perceptibly confirmed by facts when it operates over wide areas. . . . " He preached the importance of the rare hero who could break the bonds of habit and statistical regularity. Boutroux, in his dissertation, was no advocate of sociological laws standing above individuals, and confirmed by statistics. But his book, soon to be a catechism of emergentism, left an open space for just that conception of society. The lines of filiation are perfect from Boutroux to both Durkheim and to Peirce, who in 1892 was proclaiming that all the laws of nature are statistical in character.[42] Durkheim and Peirce used Boutroux's picture in different ways. Peirce, although the greatest philosopher of probability of the nineteenth century, is in many ways extremely faithful to the emergentism of Boutroux, faithful even to the idea of laws becoming encrusted in habit. For Durkheim, Boutroux had left a slightly different space to be filled.

> If the psychologist and the biologist correctly regard the phenomena of their study as well founded, merely through the fact of their connection with a number of elements of the next lower order, why should it not be the same in sociology?

The passage continues by reiterating that a belief or a social practice may exist independently of its individual expressions.

> We clearly did not mean by this that society can exist without individuals, an obvious absurdity. . . . But we did mean: 1. that the group formed by associated individuals has a reality of a different sort from each individual considered singly; 2. that collective states exist in the group from whose nature they spring, before they affect the individual as such and establish in him in a new form a purely inner existence.

11. ERROR

I have not aimed at contributing to Durkheim scholarship. Durkheim is merely one of many – I have mentioned Francis Galton and C.S. Peirce – who present us with the notion of a new kind of law of nature or society. It is a law that is either statistical on its face, or else a law that can be established only by statistical examination of mass phenomena. Peirce is concerned with the former, Durkheim with the latter. This conception of a new kind of law is part of what I call the erosion of determinism, the transition from the world of uniform and necessary laws of nature current in 1800, to the quantum world of 1936. It is also connected with the taming of chance, for it is only by making chance events law-like that we allow our faith in determinism to erode. It would be absurd to imagine any single cause of so radical a transformation in metaphysics, for the change in ideas runs through most of our branches of knowledge. It involves our central concepts, such as nature, law and cause. The transformation occurs at different rates and for different reasons in different spheres of thought and activity. Any one tale of the transformation will be full of idiosyncrasies. I have tried to relate some of the essential idiosyncrasies in one particular chain of events, hoping that other contributors to this and other volumes will illustrate other facts of related stories.

I do think that Durkheim may appear a much more inevitable figure in the light of my report. Other writers have drawn attention to the fact that many of Durkheim's ideas are foreshadowed in Brierre de Boismont's 1856 book on suicide.[43] I have not mentioned that work at all, for on reading it I found that virtually all its ideas had already been set in place. I have attempted to show the very early origin of the connection of pathology and suicide. I have traced it through the medical literature, and shown how the seemingly arcane and irrelevant classification of causes of suicide interacts with explanations of the law of large numbers. I have shown how such events made intelligible the idea of statistical laws in the moral sphere. I have shown how the debate about statistical determinism drove people to speak of tendencies that were not located in individuals. These are the eddies and backwaters of thought. It is they that made Durkheim possible. That is not a remark about the man, Durkheim, but about that form of thought for which his name is a label.

The story is instructive in another way. It has been tempting, for admirers of Durkheim, to see him as part of a progress. His predecessors were wrong about many things, but we can imagine a somewhat systematic correction of mistakes. Such is our optimistic picture of the growth of knowledge. I do not obtain such a picture from the events that I have related. I have been writing about error compounded upon error. I hesitate to say that virtually all the beliefs I have reported were false, for most of them do not seem worthy of being counted as true-or-false. There are indeed a few facts. The English have long been less given to suicide than the Franco-Germans. In the temperate zones of Europe (and America) suicide is roughly correlated with hours of daylight, so that there are more suicides in summer than winter. There are regional preferences in method of suicide. All the rest is the mythology of power, information and control. It was that mythology that brought numerical sociology into being, and it has been in the service of that mythology ever since.

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NOTES

* The present paper is an extended version of my "Suicide au XIXe siècle", pp. 168–186 of Anne Fagot (ed.) *Médecine et probabilité* (Paris: Institut de recherche universitaire d'histoire de la connaissance des idées et des mentalités, 1982), and was written in 1986. A number of themes are developed differently in my *The Taming of Chance* (Cambridge: Cambridge University Press, 1990), which includes a long chapter on Durkheim, and also a study of the idea of normalcy along lines first proposed by Canguilhem, n. 2 infra.

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[3] Emile Durkheim, "Suicide et natalité: étude de statistique morale", *Revue philosophique de la France et de l'étranger*, 1888, **13**: 446–463.

[4] Emile Durkheim, "La Sociologie en France au XIXe siècle", a lecture of 1900; see p. 113 of E. Durkheim, *La Science sociale et l'action* (Paris: Presses Universitaires de France, 1970).

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[20] J.-B. Cazauvieilh, *Du suicide, de l'aliénation mentale et des crime contres les personnes* (Paris: J.-B. Baillière, 1840), p. 16.
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[23] H. Diard, *Statistique morale de l'Angleterre et de la France par M. A.-M. Guerry: études sur cet ouvrage* (Tours: imprimerie de Laderèze, 1866): esp. pp. 4, 10.
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[31] S.-D. Poisson, "Note sur la loi des grands nombres", *Comptes rendus hebdomadaires des séances de l'Académie des Sciences* 1836, **2**: 377–382. This talk was given on 11 April 1836. The phrase "loi des grands nombres" occurs in the summary of the proceedings for 14 December 1835, *ibid.* 1835, **1**: 473–494, and it became canonical in Poisson's *Recherches sur la probabilité des jugements en matière criminelle et en matière civile, précedés des règles générales du calcul des probabilités* (Paris: Bachelier, 1837).
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[33] J.G. Spurzheim, *The Physiognomical System of Drs. Gall and Spurzheim* (London, printed for Baldwin, Craddock and Joy, 1815), pp. 491–506.
[34] E.-J. Georget, *De la folie: considérations sur cette maladie* (Paris: Crevot, 1820).

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[36] Adolphe Quetelet in a comment on p. 28 in his periodical, *Correspondances mathématiques et physiques* 1828, **4**.
[37] I. Hacking, "Prussian Numbers", pp. 377–394 of L. Krüger et al. (eds.), *The Probabilistic Revolution, Vol. 1: Ideas in History* (Cambridge, Mass.: The MIT Press, 1987).
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[39] I. Hacking, "The Autonomy of Statistical Law", pp. 3–20 in N. Rescher (ed.), *Scientific Explanation and Understanding: Essays on Reasoning and Rationality in Science* (Latham, Mass.: University Press of America, 1983).
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[41] E. Boutroux, *De la contingence des lois de la nature* (Paris: G. Baillière, 1874).
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BERNARD-PIERRE LECUYER

3. PROBABILISTIC THINKING, THE NATURAL SCIENCES AND THE SOCIAL SCIENCES: CHANGING CONFIGURATIONS (1800–1850)

THE HERITAGE OF CONDORCET AND LAPLACE

During the Enlightenment the idea progressively came to the fore that the nature of our understanding of the natural world as well as the structure of the social and political order (and of our knowledge of it) were not constituted and given once and for all. Metaphysical discussions of leading philosophers like Descartes, Leibniz, Spinoza on the fundamental nature of knowledge, or reflections on the essence of the social bond like those of Hobbes, Locke, Montesquieu, and other political philosophers, progressively gave way to the idea that the types of knowledge and of social arrangements are in a certain way temporary forms. A further step was taken when it appeared that these forms can be ordered along a succession of stages constituting a progress.

Obviously, progress can only be measured against some yardstick defining what constitutes the "natural" order, whether in the natural or in the social world, and therefore in the "social" as well as in the "natural" sciences. Thus thinkers like Hume and Rousseau continued to explore the fundamental features of our knowledge of the world and of the social order. The "relativization" and "historicization" of the world-view of the Enlighment should not therefore be over-emphasized. For example, when Rousseau defines society as an artificial condition leading man to decay and corruption, he is both historical and relativist, and at the same time a-historical and striving for an essential condition of humanity. Still what came as a deadly blow to his contemporary once fellow-*philosophes* devoted to the spreading and progress of civilization and knowledge was Rousseau's contention that such a progress represents in fact a regression.

In his recent work *Revolution in Science*,[1] I. Bernard Cohen has amply documented the fortunes of the world *Revolution* from the scientific to the social and back to the scientific spheres. Prominent in the political sphere during the Commonwealth, which was a violent upheaval (a sense quite opposed to the original scientific one), it came to be identified

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 135–152.

with moderate changes on two occasions which naturally reinforced each other: the Glorious Revolution of 1685 and, strangely enough for us, the publication of Newton's *Principia*[2] hailed by Fontenelle as a true Revolution. Newton himself however constantly insisted that his view of the world, revolutionary as it might seem, was fundamentally an improvement of the views of his predecessors. Hence his famous phrase on "the shoulders of Giants" over which he stood, thus being able to see better, a motto brilliantly commented upon by Merton.[3]

The commentators of Newton, if not Newton himself, increasingly insisted on the decisive progress introduced in our scientific knowledge of the world by his views. Similarly, after 1750 and currently during the 1780's, the static and even declining view of the social world as expressed by Montesquieu (coupled with a pessimistic empirical estimate of the trend of the world population) came to be replaced by the idea that the condition and the capacities of mankind (including its cognitive capacities and therefore its culture and its scientific knowledge) were capable of decisive improvement. This thesis was seemingly expressed first by the German playwrite Lessing in his then famous work *The Education of Mankind.*"[4] Turgot, as a future member of the clergy, expressed similar views in his discourse at the Sorbonne for the defense of his dissertation in theology.[5]

Among the late French *philosophes* who tackled simultaneously mathematical, physical and social problems, Condorcet is obviously the most prominent herald of this idea of the progress of humanity and of society through the mathematical sciences, if properly conceived and applied. His *Esquisse d'un tableau historique des progrès de l'esprit humain*, published posthumously in 1795, is the seminal work from which proceeds the entire romantic trend of XIXth century philosophy of the progress of humanity, in particular the *positivisme* of Auguste Comte (who incidentally repudiated this origin).

Condorcet shares with his younger contemporary Laplace, also member of the *Académie Royale des Sciences* of which he himself was *Secrétaire Perpétuel*, a sustained interest in probabilities. As a political and scientific adviser to Turgot, the newly appointed *Contrôleur Général des Finances* by King Louis XVIth in 1774 (head of the financial administration and *de facto* Prime Minister) Condorcet, appointed a director of the mint and of the waterways, two crucial positions, draws heavily on probabilities for at least two philosophical purposes. First, he thinks that calculus can bridge the gap between the physical and the moral sciences (this was the wording current at the time for our social sciences). Secondly, he thinks it possible to build a mathematical model on which he can establish a decision-making procedure which could be at the same time democratic, efficient, and able to reach true decisions. Hence, the particular blend of scientism, elitism and liberalism well analyzed by Baker.[6] Drawing mainly for the decision-making process on the *Essai sur les applications de l'analyse*,[7] and, for the comparison between the moral and the physical sciences on the *Tableau général de la science*[8] as well as on the *Discours de réception à l'Académie Française*[9] and the *Vie de Turgot*,[10] Baker shows convincingly the importance of the role devoted by Condorcet to probabilities in building a new social order at once rational, democratic and tolerable.

Laplace, unlike Condorcet, survived the revolutionary troubles and was thus able to convey, although in his own personal way, the convictions which he shared with his older colleague. He seems to have made a decisive impression upon Condorcet's thinking by his very early publication on probabilities.[11] The differences between the two scientists, apart from obvious differences in their career (the one brutally interrupted, the other not only extended over time but also raised to the level of the highest scientific and political positions), reside both in their scope of interest and style of thought. Perhaps even more than as a mathematician and a philosopher of probabilities, Laplace gained his worldwide fame as a physical astronomer. Near 1773 he became convinced that the theory of probabilities, as a new branch of analysis could go beyond classical geometry and lead us to a more exact knowledge of the laws of nature. In this sense, his interest in probabilities may be said to derive from his "determinism" (although the word was not in use in French during his lifetime). Ultimately, what seems important to him is not to know whether the laws of nature are necessary or contingent, deterministic or not, but to calculate their effects with the highest possible precision.[12]

Laplace's style of thinking is also strikingly different from that of Condorcet according to many historians and mathematicians. Condorcet tries to take into account in his equations all the variables and possibilities involved in the problem which he attacks. This tendency results in a convoluted style and an often unorthodox way of exposition, notation and solution of the problems. Quite to the contrary, Laplace has a unique gift to find almost immediately the correct formulation and notation; he also simplifies enough the questions raised to reach an elegant and convincing solution. This is apparently why Condorcet is often neglected by classical and modern historians of probabilities, whereas Laplace occupies a central place due to his mathematical elegance.

Laplace came back to the theory of probabilities near the end of the Empire. He published then successively the *Théorie analytique . . .*[13] which is his mathematical monument, and the *Essai philosophique sur les probabilitiés*[14] which expresses his views on this topic "*sans le secours de l'Analyse*" and applies the general principles and results of the theory of probabilities to the most important questions in life: most of them are indeed to him only probabilistic problems. Laplace constantly rewrote the *Essai* . . . until 1825: this indicates by itself the importance he attached to this publication.

A further difference between Condorcet and Laplace is worth being noted. The former never treated problems of civil life on the basis of any "real data". He preferred an abstract, more genuinely mathematical way to formulate and to solve them. Quite to the contrary, Laplace, who had obviously received superior mathematical gifts from nature, did not hesitate to tackle in 1780 and 1785 with "the natural history of man" by using sex ratios in births based on data collected in Paris, London and Naples.[15]

All this being said, the fact remains true that Condorcet and Laplace had the same ideas about the natural sciences, the moral sciences and the role of probabilities. This convergence appears in their common interest in the probability of testimonies, of jurys and the votes by the assemblies. They had two convictions: (1) The nature of the reasoning is not different in the social sciences from what it is in the natural sciences; the only difference lies in the lesser development of the social sciences; (2) Probabilities provide the general reasoning and the specific techniques by which the social sciences will reach the same degree of certainty as astronomy or the other highly mathematicized sciences.

This program was not received with extreme enthusiasm, to say the least. In particular the explorations by Condorcet, Laplace and later on by Poisson of the problem of the jurys met with ever increasing criticism. Still the very formulation of such an ambitious program for probabilities and for the sciences of man and society *(les sciences morales)* shows how far the spirit of the period was from the *Methodenstreit* which absorbed most of the energies of the German universities after 1850.[16]

An interesting and ill-fated conjunction of descriptive statistical time series and truly probabilistic analysis was the famous publication by Poisson of the *Recherches sur la probabilité des jugements.*[17] Poisson, reputed as "the preferred disciple" of Laplace, returns to the classical problem of the probability of erroneous judgments by jurys already treated by Condorcet and by Laplace. He defines himself as their heir and advocates the use of the probability calculus in the sphere of the law.

Poisson's approach to the problem consists first in revising the bold assumptions of his illustrious predecessors.[18] More revealing of the new spirit of the times is the central role which he assigns to the "law of large numbers", and in particular to the constancy of the rates or numbers over a long period of time. Already present in Laplace's writings as a consequence of the law of astronomical errors, the constancy of the rates now takes a central place, also characteristic of the writings of such statisticians as Guerry[19] and Quetelet.[20]

The sources of resistance to the application of calculus ("geometry") to moral affairs had kept all their virulence since the time of Laplace's writings. The publication of the *Théorie analytique des probabilités* (1813) and of the *Essai philosophique sur les probabilités* (1814) aroused a stern condemnation by the Pope. In the French University a leading figure of the potential opponents to the Empire, the young philosopher Royer-Collard, famous as a spokesman of the then reigning "Ideologues", thought that "Geometry does not apply to the moral order". As late as 1831, in a debate in the Chambers, Arago cites Laplace's computations as an argument again the change of 7 against 5 in the jury: there again the sitting members condemn forcefully the use of "geometry" in such matters.[21]

As for Poisson, his writings aroused the same violent criticisms against the very idea of applying mathematical models to moral and legal issues. Thus, in a session of the Academy of Sciences held in 1835, the mathematician Poinsot denounces any application of mathematics to those situations where ignorance and human passions are at stake, because the pretense to submit such an amount of irrationality to calculus would be "dangerous illusion".[22] Charles Dupin, himself a former student from Polytechnique and a member of Academy of Sciences, casts a blame on all researchers on the probabilities of judgment: they are after him hypersimplifying the complexity of the causes which interfere in judiciary decision. He too rejects the application of mathematical models to human affairs: the latter are too complex to be amenable to the simple hypotheses compatible with a mathematical analysis.[23]

The reaction against Poisson's book may be considered as a turning point: it is evidence of a growing lack of interest among "natural", "social" scientists and administrators toward the use of probabilities. On the opposite, statistics proper, based on large time series and, explicitly or implicitly, on the law of large numbers, met with increasing favor. In particular, criminal statistics collected in France after 1827 inspired not only Poisson, but also Quetelet, Guerry and many others. The idea that society was endangered by undisciplined elements, possibly whole classes of the population, often dramatized by publicists, found a striking confirmation in the revelation of the sheer number of crimes and in their perceived evolution, even on ridiculously short periods of time. Even Tocqueville commented in a dramatic tone on an increase over two years.[24]

Finally, the creation of the *Statistique Générale de la France* upon Thiers' initiative in 1833 was a consecration for the collection, analysis and publication of descriptive statistical data.[25] By then, however, Baron Fourier was dead and no one in the statistical administration could deploy a similar competence in probabilities. The times were decidedly favourable to statistics and adverse to probabilities.

QUETELET OR SOCIAL MECHANICS: TRANSFER, EXTENSION AND TRANSFORMATION OF CONCEPT

The prolific and repetitive work of the mathematically-trained social statistician Adolphe Quetelet is both central to our discussion and yet difficult to appraise correctly, partly because of its fragmented and repetitive nature, partly because of its uncompleteness and even composite and heteregeneous character.[26] Its level of mathematical and logical sophistication appears today as far inferior to that reached by such really great thinkers as Laplace, Poisson and (less coherently but much more broadly) Condorcet. Yet it is superior to the rather crude type of reasoning of most social statisticians adept of the simple numerical method discussed above (with the notable exceptions of Villermé on social mortality and Guerry on criminality and education).

In fact, Quetelet appears to belong to both lines of reasoning. He presents us with some additional paradoxes. His conception of social science as an imitation of mathematical astronomy won very few followers, but stimulated many discussions within social science. It may even be said, acording to Porter, that it was eventually more fruitful to mathematical physics than to social science itself.[27]

Although he constantly refers with praise and even reverence to the theory of probabilities as a model for social science, his specific constructs in the social sciences (*mécanique sociale, physique sociale, l'homme moyen*) all tend towards a statistical determinism based on a rather simplified interpretation of Laplace' formulation of the law of large numbers. The long way from the calculus of probabilities as a tool for analyzing and guiding individual or collective decision-making processes, to statistics as a science of statistical laws governing man's behavior and society is thus completed. Here lays one more paradox: Quetelet may be considered as one of the predecessors of Durkheim's "*sociologisme*", although Maurice Halbwachs, a prominent though not always orthodox durkheimian, has explicitly repudiated the filiation in one of his early works.[28]

Probabilities as a Model, Social Statistics as Reality

Let us come back first to Laplace. In his *Essai philosophique*... of 1814 he had written under the heading "Applications of the Calculus of Probabilities to the Moral Sciences":

> Let us apply to the political and moral sciences the method founded upon observation and upon calculus, the method which has served us so well in the natural sciences.[29]

According to Quetelet's account, it was in 1823 that he became in the social sciences one of the truest followers of this dictum. Aged only 27 at the time, he was sent officially to Paris by the government of the newly-united kingdom of Netherlands (created by the Congress of Vienna as a rampart against a return of French domination over the Belgian provinces and the Netherlands) to learn enough about astronomy so that he could direct the construction of the observatory in Brussels. This official mission to Paris was the first consecration of his meteoric career. After his dissertation in analytical geometry devoted to the demonstration of a new curve called the *focale* and immediately recognized as an important innovation (defended in 1819 when he was 24, this was the first dissertation at the newly created University of Ghent) he had been rapidly called to Brussels to occupy important teaching and administrative activities in mathematics and in what Cannon has later labelled "Humboldtian science", roughly quantitative natural history of observed phenoma like stars, tides, plants, magnetism, including population.[30]

His fateful trip to Paris changed this approach entirely. There he was received by Bouvard and introduced by him to the circle of the prestigious astronomers and mathematicians Laplace, Poisson, Fourier and Lacroix. At the end of his career, he wrote that they convinced him of the necessity to complement the study of celestial phenomena by that of the terrestrial ones, which had not been possible before.[31]

Thus, having headed to Paris to learn astronomy, he returned to Brussels a convert to statistics, then as we have seen an empirical science of society. Mathematical probability provided the bridge for his apparently surprising transition. Through the method of least squares, first proposed empirically in its perfect from by Legendre in 1805 and rapidly adopted by astronomers, mathematical probability was able to reduce variations in astronomical observations.[32] Related techniques were advocated by Laplace in his memoirs of 1785 and 1786 mentioned above, and later in the *Essai philosophique* . . . of 1814, for appreciation of accuracy of statistical measures in social science. He developed a great part of the theory of error analysis, including his use of the astronomer's curve, in the context of the measurement of social phenomena.

Quetelet constantly claimed this well-known tradition, traced by Laplace in the last section of the *Essai philosophique . . . (Notice historique sur le calcul des probabilités)* and by Quetelet himself[33] as that of his teachers and models. However, these self-assigned models usually concentrated their work on the most highly mathematicized problems such as the calculation of life tables for the insurance companies and annuities. For that reason they rarely ventured beyond mathematical problems of mortality and population.

Apparently Quetelet shared these views in his very first statistical paper published in 1826 and presented as a contribution to the study of insurance.[34] But even in this highly mathematicized domain of population, Quetelet devoted a great part of his attention to questions pertaining to Malthus' theory, which was never considered worth of consideration by the mathematical astronomers who were developing the calculus of probability. Apart from this specific example, it is clear that Quetelet wanted to create an updated version of the old political arithmetic as well as to the German descriptive science of state or *statistik*, that would be a necessary tool to the legislator, and which would include such matters as human growth, crime, education, agriculture, commerce, industry, population matters, etc.

Naturally when the Dutch government, on the model of the French developments discussed above, established a Royal Statistical Commission, he became its correspondent for Brabant. It is in this capacity that he published his first paper on mortality tables and insurances. In 1827 he analyzed crime statistics, with a practical eye to improving the administration of justice. In 1828 he edited a general statistical handbook separately on Belgium, with comparative material obtained from colleagues he had come to know during his stays in France and also in England. At his urging, a census of population was taken in 1829, the results of which were published separately for Holland and Belgium after the revolution of 1830–31 and the independence of Belgium in 1833. In 1841, largely through his efforts, the *Commission centrale de Statistique* was organized and rapidly became the central agency for the collection of statistics in Belgium: under his presidency it came to be regarded as a model by statisticians in other countries. His decisive role in the creation of the Statistical Society of London in 1834 and of the International Statistical Congresses in 1853 is to be mentioned in this same line of activities, and too well documented to need to be recalled in detail.[35]

It is however striking to note that in all the volumes of social statistics that he wrote, he never made any actual use of probabilities such as a simple computation of standard error. Like the other social statisticians whose work he acknowledged to be of high quality, like his friend and correspondent Villermé, Benoiston de Châteauneuf and William Farr, his claims for the scientific character of his enterprise were based on the proposition that complete and reliable enumerations produce facts – not probabilities. Still it must be recalled that the technique of surveys based on probabilistic sampling does not appear until the end of the XIXth century under a rudimentary form, more intuitive than formalized, with the Norwegian statistician Kiaer. The first computations of confidence intervals are realized by the English Bowley in 1906, and the detailed formalization of stratification methods is advanced by Neyman only in 1934 (Desrosières, 1988). At that time sampling methods were widely used in mathematical physics since a long time. Yet Quetelet's complex metaphors of social physics and his constant references to the crucial importance of probability for statistics reveal his project of unifying these two separate traditions, of making "the urgent business of social reform as scientifically profound as the *Mécanique céleste*, as well-ordered as the *Système du monde*".[36]

THE TRADITIONALISTS, SAINT-SIMON AND AUGUSTE COMTE: FOUNDATIONS FOR A DETERMINISTIC SOCIAL SCIENCE

Another trend must be mentioned in this review of the configurations between the social sciences, the natural sciences and probabilistic or deterministic thinking. This is the so-called "traditionalistic" school of social philosophy, also labelled with praise by Auguste Comte as *"l'école rétrograde"*.[37] Against the contractualistic theory of society which goes roughly from Rousseau (at least in French) to Condorcet and to the *Idéologues* and against the analytical notion of language as a derived and constructed social link, the traditionalists put forth two central ideas.

First, power, sovereignty and society have "natural", not "contractual" origins. For Maistre, "natural" means in effect "divine". That is, society and language preceed the individual: they are the agents which constitute him as an individual, not the opposite. The whole predominates over the parts. In this respect the traditionalist mode of thinking has deep similarities with organicism, as Koyré notes in his study on Bonald.[38] Still no direct association with the life sciences may be detected.

Second, it is language which creates society by its communicative capacities, not the opposite. For the philosophers and the *Idéologues* language is the derived product of society. But how can society exist without language? Therefore language, the traditionalists argue, has necessarily its origins outside society. Since man exists as such only within society, the origin of language must be traced beyond man. This is Bonald's celebrated and central thesis of the "divine origin of language". Bonald is the most dogmatic and deductive theoretician of the school, Maistre being also theoretical but more empirically oriented; Chateaubriand and Lamennais are more religiously inclined propagandists.[39]

The traditionalistic school made itself heard as a staunch counter-revolutionary trend very early in the revolutionary process. Its first two works were published in 1796. Maistre's volume had a great success in France; at the request of the Directorate, the Helvetic Confederacy (where the book had in fact been published) expulsed the author who began his long career as a proscript. On the contrary, Bonald secured a strong position under the Consulate, the Empire and the Restoration and played throughout the period a key political role which may be seen as counterbalancing that of Laplace.[40] At any rate Maistre's and Bonald's influence over Comte was great and exerted itself in a direction adverse to probabilistic thinking.

Another configuration linking the social sciences with the life sciences in a deterministic and anti-probabilistic direction is represented by Saint-Simon. This is all the more remarkable since in his early writings he had referred to Newton's gravitation law as an all-encompassing and general organizing scheme for the unification of science. In his next publication he approvingly cites Condorcet, whose work represents to him at that period the most advanced stage of our knowledge so far, but invites writers to go beyond him.

The shift in the direction of the life sciences substituted for the law of Newton as the source of the basic principle for unifying the science of man occurs in 1813. In this work Saint-Simon, through the statements expressed by his character Dr. Burdin, tries to specify the famous "unity of the knowledge of the human being" postulated in his first publication. It is there (1813) that he launches the expression of "social physiology" or physiology as a "positive science" which is still to be realized despite the (still fragmentary) contributions of Vicq d'Azyr, Cabanis, Bichat and Condorcet. Incidentally Isambert takes argument of the continued reference to Condorcet to contradict Gouhier's assertions that after 1813 Saint-Simon conceives his "science of man" "according to Cabanis".[41]

On page 31 of the *Mémoire* is expressed the famous address to the mathematical scientists which reads as follows:

> Brutalists (*brutiers*), infinitesimalists, algebraists and arithmeticians, what right do you have to be posted at the scientific vanguard? The human species is engaged in one of the most severe crises it has endured since its origins: what effort are you doing to terminate this crisis? What means do you have to reestablish order in human society? The knowledge of man is the only one that can lead to discover the means to conciliate the interests of the peoples, and you do not study this science . . . Quit the direction of the scientific workshop; leave it to us to warm its heart frozen under your presidency, and call back all its attention to works which can restore the general peace by reorganizing society (*My translation*).

What were the actual references of Saint-Simon to physiology? He first referred to physiologists he had personally known, namely Vicq d'Azyr,[42] Cabanis[43] and Bichat,[44] but to earlier writers like Barthez[45] and through the works of Bordeu[46] back to Haller. Comte being closely associated with Saint-Simon for almost ten years was naturally aware of these developments to which he himself contributed largely in his own capacity. Thus a special relationship was established between physiology and sociology at its beginnings, involving a negative overtone towards probabilistic thinking, much against the hopes expressed by Condorcet.

We have explored Quetelet's central although frequently inconsistent position. It is quite unclear whether his approach to statistical regularities is inherently probabilistic or deeply deterministic, although he constantly refers with praise and reverence to celestial mechanics. This reverence itself is ambiguous, since celestial mechanics implies at least the use of probabilistic techniques, which Quetelet himself never utilized. On the other hand, he infused these techniques with his own social conceptions, thus paving the way for further developments in mathematical statistics and statistical physics, whereas his conceptions of social physics met almost immediately, mostly in Germany[47] but also in France and Great-Britain[48] with virulent criticism from vital and social statisticians.

Comte stands to an almost symmetrical opposition to Quetelet. His positions – which were deeply influential in the social but also in the life sciences – established a distance, even a barrier between these sciences and probabilistic thinking. Within the social sciences themselves, sociology (Comte's own child) was established as both a domain and a style of thought opposed not only to probabilistic thinking proper, but also ignorant of statistical regularities and thus cut apart from psychology (Durkheim tended to perpetuate this scission) and demography. I shall examine in turn 1) the contrast between Comte and Quetelet; 2) the source of their divergence: Comte's opposition to probabilistic reasoning; 3) the biological bases of Comte's antiprobabilistic philosophy.

A parallel is often drawn between Quetelet and Comte. This all too often perfunctory exercise has been thoroughly renewed by an essay from professor Julien Freund[49] who seems decisive in several respects. Freund notices at the outset that mutual appraisal and appreciation could have been expected from writers belonging to the same generation (Quetelet was born in 1796, Comte in 1798) having in addition a similarly strong background in the natural sciences (mathematics and astronomy) and a grossly similar ambition: the application of the methods current in the natural sciences to the study of social problems. Their careers, however, were in complete opposition, Quetelet accumulating scientific successes, honors and gaining wide scientific recognition whereas Comte constantly met with the hostility of the scientific establishment. More precisely, their paths crossed each other in 1838, when Comte expressed in the 46th Lesson of the *Cours de philosophie positive* his fundamental divergence with Quetelet over the expression "*physique sociale*". This was used by Quetelet as we have seen in 1835: it appears in the title of his book published this year, apparently with great success – Comte had largely preceded Quetelet in this respect, since he had used "*physique sociale*". as early as 1822 as a substitution to Saint-Simon favored expression "*physiologie sociale*". Comte considered in the *Plan des travaux scientifiques nécessaires pour réorganiser la société* that his "*physique sociale*" was called to absorb the "*physiologie sociale*" as defined by his old master; several of his writings of 1826 and 1828 develop this idea.

What he does in 1838 is to recall in a contemptuous way his priority over Quetelet in putting forward the ideas and expression "*physique sociale*" and to distinguish very carefully between his own acceptance of the terms and that of Quetelet which he disdainfully characterizes as an "abuse committed by a Belgian scholar who adopted it, these last years, as the title of a work dealing at best with statistics". Then in the next *Leçon* he proposes as a more convenient substitute to "*physique sociale*" his later triumphant neologism "*sociologie*" with excuses for introducing a neologism although he had opposed in principle such a practice.

In this part of his development Freund seems to have made his point that the legend of the origins of the word "*sociologie*" is not entirely accurate. Comte did not coin it because "*physique sociale*" was pre-empted by Quetelet. He was the inventor of "*physique sociale*" as well. What he did was to recall his priority, and then to offer what seemed to him a more convenient substitute.

The divergence between Comte and Quetelet over the meaning to be attached to "*physique sociale*" is only the most salient consequence of a wider and obvious opposition as to the place of probabilities in scientific knowledge and in the theory of causality, itself covering a more profound opposition in their philosophy of knowledge, whether of nature or of society.

Early in his writings Comte departed significantly from Saint-Simon's thought. Not only did he substitute social physics to social physiology as one of the six fundamental sciences, but he suppressed any reference to Condorcet and sharply opposed the use of probabilities in either the biological and medical or the political and social sciences. As early as the mid-1820's he states that pretensions of such applications are "chimerae", that they are "anchored in the metaphysical prejudice". This applies evidently to politics and to political science:

The nature of political phenomena forbids absolutely any hope of ever applying to them mathematical analysis; (that) such an application, on the other hand, suppose it be possible, would be null in helping raise the political sciences at the rank of the positive sciences, since it would require, to be feasible, that science in general should be fully constitued *'puisqu'elle exigerait, pour êre praticable, que la science fût faite'* (my translation).

In the 40th lesson of the *Cours de philosophie positive*, the case of the medical sciences is considered, quite in similar terms:

The spirit of calculus tends on our days to introduce itself in this study, mostly in what concerns the medical sciences, by a much less direct way, under a more specious form, and with infinitely more modest pretentions. I mean, this supposed application of what is called statistics to medicine, from which several scientists (*'savants'*) expect wonders, and which however can but end, by its very nature, in a deep degeneration of the medical art, instantly (*'dès lors'*) reduced to blind numberings. Such a method if it is permitted to grant it this name, would not really be anything else but absolute empiricism, disguised under frivolous mathematical appearances (idem).

The same judgment is reiterated in the 49th lesson, about sociology:

The only aberration of this type which could have deserved some serious discussion, if this whole Treatise had not in advance radically dispensed us with it, is the vane pretense of a great number of geometricians to render the social studies positive from a chimerical subordination to the illusory theory of the chances.

He then directs his attacks not against Quetelet, but against Condorcet whom Saint-Simon had hailed as a precursor, and stigmatizes any application of mathematics, and mostly of statistics, to the social sciences as an "aberration" since "signs are habitually (mistaken for) ideas, after the usual character of purely metaphysical speculations".

The fundamental reason why Comte was hostile to probabilities is to be sought in the biological bases of his philosophy of knowledge. Comte itself was so conscious of the fact that he explicitly attributed the numerous setbacks of his scientific career to his desertion of the then reigning mathematical school. Although professionally trained a mathematician, he ranked himself alongside with the biological school, fighting, as he writes, to maintain:

against the irrational ascendance of the mathematical school, the independence and the dignity of the organic studies (my translation).[50]

We know that sociology is introduced in the 47th lesson of the *Cours de philosophie positive* as the crowning piece of the whole edifice of knowledge. But this is possible for Comte only because biology has allowed him (40th lesson of the *Cours*) to introduce a total methodological reversal; with biology knowledge moves from the whole to its parts, whereas the previous methodological order inspired by astronomy went from the parts to the whole. The reversal is authorized, more: it is required by the very notion of organism implying the subordination of what is simple to what is complex, of the beginnings of the hierarchical series of beings to its completion. He later calls this dramatic change:

> (the great) scientific revolution which, under Bichat's impulse transports from astronomy to biology the general presidence of natural philosophy.
>
> (*Système de politique positive*, I)[51] (my translation).

According to his hierarchical conception of knowledge, sociology tells the truth for biology, which explain physics. "Sociology, not an illusory psychology, gives us the key to a veritable theory of intelligence".

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NOTES

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[49] Julien Freund: "Quetelet et Auguste Comte", op. cit.
[50] Auguste Comte: *Cours de philosophie positive*, (Paris: Bachelier, 1830–1842, 6 vols).
[51] Auguste Comte: "Plan des travaux scientifiques nécessaires pour réorganiser la société" in *Système de politique positive: ou Traité de sociologie instituant la religion de l'humanité* (Paris: Mathias, 1851–1854, 4 vols, vol. IV: 47–136).

I. BERNARD COHEN

4. THE SCIENTIFIC REVOLUTION AND THE SOCIAL SCIENCES

4.1. THE "NEW SCIENCE" AND THE SCIENCES OF SOCIETY

Ever since the great revolution which produced modern science there has been a hope that a science of society would be created on a par with the sciences of nature. Two early heroes of the Scientific Revolution, Galileo and Harvey, created radical transformations of science – respectively, a physics of motion and a physiology based on the circulation of the blood – which became paradigms for a new social science.[1] Scientific precepts of Bacon and of Descartes were available as guides in this new venture. A primary challenge was to accommodate a new social science to mathematics: either to use classical mathematics for a non-traditional purpose or to introduce a kind of mathematics other than geometry on the Greek pattern. Would-be social scientists could thus find novel ways of dealing with their subject that would transfer to their endeavors the authority of mathematics and the new natural sciences.

In the pre-Newtonian part of the "century of genius" – in the decades that encompass the careers of Galileo, Kepler, Harvey, Bacon, and Descartes – there were a number of earnests of the desired new science of society. Later on in the seventeenth century and during the succeeding century of the "Enlightenment," Newton's spectacular achievement in the *Principia* aroused hopes for a similar science[2] of man and of society, a "human science" of individual behavior and a "social science" of the behavior of large groups. From that day onward, social scientists have been waiting patiently (and sometimes even impatiently) for their "Newton."[3] The history of the social sciences plainly shows that neither the rational mechanics of Newton's *Principia* nor the Newtonian system of the world has ever served successfully as a direct model for engendering a similarly constructed social science.[4] And so, in considering the impact of the natural sciences on the social sciences in the seventeenth century, we shall focus our attention exclusively on the pre-Newtonian decades, taking note of attempts to develop a "science" of government or of the state. We shall examine

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 153–203.

topics that would later become parts of sociology, political science, economics, or the study of the law. Discussions in all of these areas were to some degree influenced by the revolutionary advances in mathematics and in the physical and biological sciences.

In the early age of the Scientific Revolution, the greatest and most obvious accomplishments were to be seen in the "exact sciences" – mathematics (Descartes, Fermat, and also Galileo), astronomy (Galileo, Kepler), and the physics of motion (Galileo, Descartes, and also Kepler). A comparable revolution in the life sciences was the discovery by Harvey of the circulation of the blood. The mathematical achievements were outstanding because they represented a great conceptual revolution: a new way of thinking based on algebra and analysis rather than the traditional synthetic geometry. The innovations of the new astronomy were both conceptual and observational. Galileo's use of the telescope wholly altered the observational basis of knowledge of the universe, while Kepler introduced non-circular orbits and the concept of sun-planet forces. The most basic alteration in physics occurred in the study of motion, which entailed new conceptual foundations and a mathematicization of nature, to a much greater degree than direct questioning of nature by experiment. From today's point of view the most fundamental change during the early 1600s appears to have been the destruction of the Aristotelian cosmos, the rejection of the traditional concept of the hierarchical nature of space, and the introduction of the new idea of isotropic space, inertial physics, and an infinite – or at least unbounded – universe.[5] The major innovation in the life sciences centered on a radical discovery of the circulation of the blood, based on a conceptual shift made necessary by the introduction of quantitative considerations. Thus the revolutionary changes in science did not consist primarily of the introduction of experiments, as was long believed by historians, but rather was premised on a basic shift of intellectual framework centering on new concepts and the introduction of new mathematical methods.

I have mentioned that Galileo was one of the great heroes of the early Scientific Revolution. In publications, he proclaimed his official position as "philosopher and mathematician." This title accurately recorded the two distinct kinds of science on which his fame was based: empirically based natural philosophy and mathematical science. As empiricist, Galileo was the astronomer who, using the newly invented telescope, showed that the Earth is like the moon and the planets and not unique, thus making the Copernican system philosophically reasonable and hence worthy of serious scientific consideration. His study of the phases of Venus proved that the Ptolemaic system is false.[6] It was as an empiricist that Galileo won renown for experiments of dropping unequal weights from a tower so as to prove that a principal Aristotelian tenet about motion is wrong: bodies do not fall freely in air in the way that Aristotelians supposed.[7] His greatest contribution to physics, however, was not experimental but intellectual: to set forth a new way of thinking about motion, analyzing the problems of natural uniform and accelerated motion in terms of new and clearly defined concepts and principles which he used to develop mathematical laws about speed, distance, and time.[8] In his great book on *Two New Sciences*, Galileo set forth his results concerning motion in a mathematical framework, derived in a geometric style from fundamental definitions and principles. His readers thus saw Galilean physics set in a mathematical structure and did not consider this subject as having been derived from, or even based primarily on, direct experiment.[9] Into the framework of mathematical deduction, Galileo introduced mathematical postulates of physics suited to a new science. Above all, Galileo demonstrated the power of mathematical reasoning applied to abstract or imagined systems that were derived from nature simplified, a method later brought to a high level of fruition by Isaac Newton in his *Principia*.[10] This mathematical method enabled Galileo to transcend the difficulties of the complex physical world of nature by achieving solutions for the ideal case; later, he could introduce some factors of "this" world of "reality." Although Galileo did not essay an application of his science to problems in the social or political arenas, his mathematico-physical model was greatly esteemed by those who strove to produce a mathematically based analysis of social or political affairs.

Along with Galileo, Descartes was generally held in high esteem by social scientists and philosophers during the early years of the Scientific Revolution. Descartes was the author of major works on geometry, optics, and the atmosphere and was a champion of the "mathematical way." He was recognized as a primary founder of the new mathematics, a pioneer in the theory of equations, and an inventor of a new kind of geometry based on algebra, an honor which he shares with Fermat. Descartes was also the author of the celebrated *Discourse on Method* (1637), which was a rival to the precepts of Bacon. Like Bacon, Descartes predicted that the pursuit of natural science would enable human beings to control their environment. His *Principles of Philosophy* embraced the physics of motion (based on the principle of inertia), principles of cosmology, a system of the world, and general philosophy. Additionally, Descartes presented a radical new "science of man," in which all human functions were to be reduced to mechanical actions. This was part of the general "mechanical philosophy," in which nature's operations were to be explained by two "principles": matter and motion.[11]

Early in his career Descartes had a dream which revealed to him the "foundations of the Admirable Science," the way in which he could use the infallible method of mathematics to solve problems of science and philosophy. He envisaged a "universal mathematical science" and even hoped to produce a geometric ethics, a project that he believed might be simpler than constructing a mathematical medicine or physiology.[12] Descartes's human science also drew on his personal experience in making and observing dissections of animals. Furthermore, he devoted a considerable portion of part five of his *Discourse on Method* to a presentation of Harvey's discovery of the circulation of the blood and praised Harvey for his use of observation and experiment.

Harvey's discovery of the circulation of the blood was in keeping with the mathematical spirit of the age, at least to the degree that his great discovery was based on mathematics as well as on a broad range of empirical investigations. Mathematics in the form of quantitative reasoning gave Harvey an early insight into the need for a new physiology and provided a powerful argument for his ideas about the circulation. Harvey's path to discovery, like his presentation in the *De Motu Cordis* of 1628, was solidly based on anatomical investigations (including a great variety of direct observation and experiment), notably in uncovering the function of the valves in the veins and the structure and action of the heart. But readers of *De Motu Cordis* could not help but be impressed by his calculations, which proved that Galen's physiology is inadequate. Harvey found that "the juice of the food that had been eaten" simply would not suffice for the liver to supply "the abundance of the blood that was passed through" the heart. And so, Harvey wrote, "I began to bethink myself" whether the blood "might not have a kind of movement, as it were in a circle." And this, he declared, "I afterwards found to be true."[13]

Harvey's conception of the circulation of the blood was a tremendous advance in human science. He showed that the heart with its valves acts in the manner of a water pump, forcing the blood to flow in a continuous circuit through the animal or human body. This was a direct affront to the prevailing doctrine of Galen, which had dominated medical and biological thought ever since it had been propounded fifteen centuries earlier. Galen had given prominence to the liver as the organ which continually manufactures blood to be sent out through the body and used up as the different parts perform their life functions. But Harvey shifted the physiological primacy of organs from the liver to the heart, whose function, he said, was to a large degree mechanical, forcing blood out through the arteries and drawing blood in from the veins.

Harvey differed from Descartes and Galileo in conceiving that his important scientific discovery could have a direct paradigmatic value in the domain of social affairs. In introducing his great work *De Motu Cordis*, Harvey used his new science of the body to transform the old notion of the body politic. This dramatic example of the use of the new science in a socio-political context occurs at the very beginning of the book, in the long and flowery dedication to the reigning king, Charles I. The following passage expresses Harvey's view unambiguously:

> The heart of creatures is the foundation of life, the prince of all, the sun of their microcosm, on which all vitality depends, from whence all vigor and strength arises. Likewise the King, foundation of his kingdoms and sun of his microcosm, is the heart of the commonwealth, from whence all power arises, all mercy proceeds.

Harvey had no question but that "almost all things human are according to the pattern of man" and "most things in a King are according to that of the heart." Hence "knowledge of his own heart" must be profitable "to a King, as being a divine exemplar of his functions," in accordance with the customary comparison of "great things with small." Since Charles was "placed at the pinnacle of human things" he would be able to "contemplate at one and the same time" both the "principle of man's body" and "the image" of his own "kingly power."[14]

When Harvey wrote of the king's acquisition of knowledge of the heart and its functions, he must have had in mind that Charles had indeed become aware, through Harvey, of this aspect of physiology. Harvey knew Charles personally as a royal physician, and it was through Charles's direct intervention that deer from the royal herd were made available to him for his studies of animal generation. Harvey not only personally instructed the king about the heart and the circulation, as well as about his discoveries in embryology, but he recorded in his *De Generatione Animalium* how he had shown Charles a "punctum saliens" or pulsating point in the uterus of a doe.[15] The king's genuine concern for Harvey's studies of the heart led to the only occasion on which Harvey could actually examine a live human heart beating. After Charles had heard that a son of the Viscount Montgomery had suffered a chest wound that resulted in a permanent open fistula or cavity, permitting direct vision of the interior organs, he instructed Harvey to make a personal examination of the young man. Harvey examined him and was so greatly impressed that he arranged for the young man to be brought to the royal court in order that the king and Harvey might watch the movement of the heart and touch the ventricles while they contracted and expanded, as Harvey himself had already done on his own. Charles was said to have remarked to this young man, "Sir, I wish I could perceive the thoughts of some of my nobilities' hearts as I have seen your heart."[16]

Harvey's comparison of the role of a king and the function of the heart is cast in a traditional mode of thought, the ancient organismic analogy of the "body politic," in which the state was compared to an animal or person, and the sovereign was considered to be the head ruling the body. Some earlier presentations of the body politic used the concept of the heart as a ruler, but others placed the head in this role in the usage still current in our concept of the "head of a state."[17] A few writers on the body politic prior to Harvey had given importance to the heart, but in a framework of Aristotelian or Galenic thought. Thus, in 1565, the surgeon John Halle, who held that "the harte of man [is] a king," declared "the lyver" to be one of "the chief governours under hym," referring to the Galenic principle that the liver is constantly generating new blood from digested food and sending it to the heart.[18] But in Harvey's system the liver was relegated to an inferior position as a result of his own discovery that the blood circulates through the mechanical pumping action of the heart rather than being constantly generated by the liver.[19]

The sovereignty of the heart is a feature of Aristotelian physiology, which even includes the assertion that in the developing embryo the heart is formed before the blood. Harvey's embryological investigations showed, however, that the blood comes into being prior to the embryo heart or other organ, thus revealing the nature of the "punctum saliens" – a feature of the development of the embryo that was to acquire significance in the context of political theory in the writings of James Harrington. Harvey's views on the heart consequently have two features. In his *De Generatione Animalium* the heart is relegated to an inferior position in that it does not appear as the first discernible part in the development of the embryo, but in *De Motu Cordis* the heart acquires a primacy because of its fundamental role in pumping the blood through the animal body. In *Exercitationes Anatomicae de Generatione Animalium* (1651) Harvey made the distinction clear:

> And so being made more sure by those things which I have observed in the egg and in the dissection of living animals, I maintain, contrary to Aristotle, that the blood is the first genital particle, and that the heart is its instrument designed for its circulation. For the function of the heart is the driving on of the blood. . . . [20]

Even when Harvey compared the role of the monarchy to the function of the heart he was not interpreting the heart's primacy in the traditional Aristotelian sense.

Lest it be thought that Harvey introduced the body politic only in the dedication of *De Motu Cordis* and not in the context of his scientific presentation, let me hasten to add that this theme appears again in the text itself, in the concluding chapter seventeen, in which Harvey proves "the hypothesis of the movement and circulation of the blood" by reference to the observable phenomena of the heart and the evidence of "anatomical dissection." The heart is the first organ of the body to appear in a complete form in the developing embryo, Harvey wrote, and it "contains within itself blood, life, sensation and motion before either the brain or the liver was made" or "could perform any function"; to this degree the heart is "like some internal animal." The heart, furthermore, Harvey then declared, is "like the Prince in the Commonwealth in whose person lies the first and supreme power." The heart "governs all things everywhere, and from it as from its origin and foundation in the living creature all power derives and on it does depend."[21]

Harvey's transformation of the traditional organismic analogy of the state (the "body politic") in the context of his own discoveries sanctioned further explorations of political systems based on the new human physiology. Thus the inaugurator of modern physiology introduced his founding treatise with a bold declaration that true science is related to the functioning of the state. I know of no similar statement by any other founder of the new science. Such a sentiment would perhaps come more naturally to Harvey than to a Galileo or a Kepler because the fabric of the human body shows the same kind of complex organization and varied interaction of parts that is found in organized humanity.

4.2. THE SEVENTEENTH-CENTURY GOAL OF A SOCIAL SCIENCE IN MATHEMATICAL FORM (GROTIUS, SPINOZA, VAUBAN)

During the first flowering of the Scientific Revolution in the early seventeenth century, mathematics was the area of most easily discernible achievement. Hence, it is hardly surprising that during the first great century of the Scientific Revolution there were attempts to duplicate the success of these mathematical pioneers by producing a new science of the state or of society in a mathematical mold.

Emulation and application of mathematics took four major forms. The first and perhaps the foremost was the aim of producing works that would display the clarity and certainty of mathematical reasoning, that would be as infallible as Euclidean geometry. The second was the attempt to adopt the actual structural form of presentation: ordered sets of definitions, of axioms and postulates, leading to proved theorems. The third was to apply new mathematical techniques and methods, such as those of algebra and shopkeeper's arithmetic, in order to produce a moral or ethical calculus or a form of social or political mathematics. The fourth was to use numerical social data in the manner that had proved successful in the physical or biological sciences; a corollary was to encourage the collection of such numerical data for this purpose.

The goal of emulating mathematics in creating a new social science may be seen in the thought of Huig (or Huigh) de Groot, or Hugo Grotius (1583–1645), one of the founders of modern international law. Grotius is a particularly significant figure in this context because his reputation was made as a scholarly jurist and his career is not usually associated with the mathematical sciences of the seventeenth century. But in 1636 Grotius corresponded with Galileo in relation to the latter's proposal of a new means of determining longitude at sea, a subject familiar to Grotius, since he had translated (from Dutch into Latin) a work on this topic by the Dutch engineer, Simon Stevin, who was also a friend of his father's.[22] In his letter to Galileo, Grotius expressed his enormous admiration for Galileo's accomplishments, which, he said, "surpass all human endeavor and bring it about that we neither need the writings of the ancients nor fear that any future age will triumph over this one." He would not wish, he continued, "to take to myself the glory of claiming to have been one of your disciples, for it requires great ability to reach that level even when you lead the way." But, he wrote, "if I claim to have been always one of your admirers I will not be speaking falsely." He made his main point in a poetic vein: "I will be happy if in any way I can serve as midwife to your offspring as they come forth into the light of immortality."[23]

Grotius's admiration for a Galilean mathematical physics may be detected in his treatise of 1625, *De Jure Belli ac Pacis*, or *Law of War and Peace*, the work on which his fame was built. In the *Prolegomena* he declared that in writing his treatise he had not considered "any controversies of our own times, either those that have arisen or those which can be foreseen as likely to arise," and he insisted that in this regard he had followed the procedure of mathematicians ("mathematici"). "Just as mathematicians treat their figures as abstracted from bodies," he wrote, "so in treating law I have withdrawn my mind from every particular fact." Grotius evidently believed that his science of international law was as sound and secure as any system of mathematics because he had adopted the same high level of abstraction and accordingly had divorced himself from actual events. He held that his "proofs of things touching the law of nature" were based on "certain fundamental conceptions which are beyond question, "so that no one can deny them without doing violence to himself."[24]

Already in *De Jure Praedae Commentarius*, composed in 1604–1606 although not published in full until 1868, there occurs a statement about "mathematicians" which, despite a slight difference in signification and application, is nevertheless very close to the statement made about "mathematicians" in the famous work published in 1625. In the first chapter of the earlier text, begun when Grotius was only about twenty-one years of age and formulated as a legal brief addressing a particular contemporaneous crisis, the youthful but learned jurist explained his method:

> Just as the mathematicians customarily prefix to any concrete demonstration a preliminary statement of certain broad axioms [*communes quasdam . . . notiones*] on which all persons are easily agreed, in order that there may be some fixed point from which to trace the proof of what follows, so shall we point out certain rules [*regulas*] and laws [*leges*] of the most general nature, presenting them as preliminary assumptions which need to be recalled rather than learned for the first time, with the purpose of laying a foundation upon which our other conclusions may safely rest.

The application of this method in *De Jure Praedae* has been concisely described by Ben Vermeulen:

> The second chapter contains the premisses in the form of nine definitions (*regulae*), in which types of law are described in terms of the gradations of will expressed in a hierarchy of lawgivers, and thirteen precepts (*leges*), which flow from these *regulae*.

Subsequently, various propositions (*conclusiones* and *corollaria*) are derived from the definitions and precepts (chapters III–X). In chapter XI there follows an historical account of the case, which is judged in the light of the *conclusiones* and *corollaria* (chapters XII and XIII . . .).

Thus Grotius is to a certain extent using a method which may be characterized as mathematical or geometrical even if it cannot be regarded as physico-mathematical or arithmetical or quantitative.[25]

Similarly, when Grotius wrote in *De Jure Belli ac Pacis* that he conceived the science of the law of nations in a mathematical mode, he did not intend that law should be given a quantitative base. Rather he meant, as he said, that he would follow a rational procedure: "In my work as a whole I have, above all else, aimed at three things: to make the reasons for my conclusions as evident as possible; to set forth in a definite order the matters which needed to be treated; and to distinguish clearly between things which seemed to be the same and were not." In addition, the Polish scholar Waldemar Voisé has pointed out that in analyzing the concept of justice Grotius adduced "geometrical and arithmetic proportion" and held that for mathematicians "comparative or geometrical" justice "has the name of proportion."[26] Furthermore, conceiving of nature as unalterable, Grotius assumed that neither man nor God could interfere with the necessity of nature's laws. Drawing an example from mathematics, he declared that God himself could not make two times two be anything but four and that God could not alter what had to be in the domain of natural right and natural law. This is akin to a conclusion which Grotius himself recognized as verging on blasphemy, that natural right could exist even if there were no Supreme Being.[27] Grotius thus "freed the concept of natural law from its heteronymous, divine origin" and reduced it to "an element of human nature that can be known by the exercise of reason, in a manner like that which characterizes the rules of mathematics."[28] It may be at least partly because Grotius conceived his system in a mathematical mode and therefore referred to abstractions rather than to real contemporaneous or historical events that he has been criticized as unrealistic by those who have not appreciated the reason for this framework.[29]

The mathematical context of Grotius's work on international law does not receive much attention from today's authorities. Mathematics is not even mentioned in the article on Grotius in the current *International Encyclopedia of the Social Sciences* (1968) or its predecessor, the *Encyclopaedia of the Social Sciences* 1935). In at least one English translation of Grotius's classic work on war and peace, the *Prolegomena* (containing the most explicit discussion of Grotius's mathematical method) are omitted altogether.[30]

To us the characterization of a treatise as mathematical entails either the use of the commonly recognized techniques of mathematical analysis or the introduction of numbers and quantitative data. Accordingly, Grotius's *De Jure Belli ac Pacis* does not appear to us to be mathematical. But in the century of the Scientific Revolution and in the Enlightenment, scholars held the mathematical aspect of Grotius's method to be of the greatest significance. The jurist Christian Thomasius, who published in 1707 a German version of *De Jure Belli ac Pacis*[31] maintained that Hugo Grotius, Thomas Hobbes, and Samuel Pufendorf had distinguished themselves by using the mathematical mode of reasoning about natural law. Thomasius even went so far as to declare that a person who was not a mathematician could never hope to understand the science of natural law. Pufendorf himself explicitly declared that the true science of law had begun only with Grotius and Hobbes, for the reason that they had introduced mathematical reasoning into this subject.[32]

The most celebrated example of the geometrico-mathematical mode of discourse in the age of the Scientific Revolution is the *Ethics* (completed in 1674, but not published until 1677) by Benedict Spinoza (1632–1677), of which the full title reads *Ethica Ordine Geometrico Demonstrata*. Set in a strictly Euclidean framework, this treatise begins with a set of eight numbered definitions and axioms, leading to numbered propositions and their proofs. Later on, there are other sets of numbered definition and axioms, leading to additional propositions and proofs. There are also postulates and lemmas.[33] But although the external form is strictly geometrical or in the style of Euclid (*more geometrico* or *ordine geometrico*), Spinoza does not use the actual techniques of mathematics or geometry in the development of his subject. Nor does his argument in any way depend on numerical data or quantitative considerations.

Spinoza did not employ this geometric form in his other works. But in the *Treatise on Politics* he claimed that he had adopted "the same objectivity as we generally show in mathematical inquiries."[34] That is, in grounding politics on "the real nature of man," he had "taken great care to understand human actions, and not to deride, deplore, or denounce them." In short, he wrote,

I have therefore regarded human passions like love, hate, anger, envy, pride, pity, and the other feelings that agitate the mind, not as vices of human nature, but as properties which belong to it in the same way as heat, cold, storm, thunder and the like belong to the nature of the atmosphere.[35]

Another example of the application of a geometric method to a problem in the social sciences was an essay by Gottfried Wilhelm Leibniz (1646–1716) on the choice of a king of Poland. Titled *Specimen Demonstrationum Politicarum* (specimen or model of political demonstrations), this little work proclaimed through its subtitle that Leibniz had used "a new style of writing intended to produce clear certainty." Published in 1669, eight years before Spinoza's *Ethics*, Leibniz's *Specimen* differs from all similar efforts of that age because his goal was to solve a particular political problem, not to construct an abstract general system.

The *Specimen* is also of interest because it contains a suggestion of a logical calculus of probabilities – in a political context. Although the *Specimen* is not mentioned in many works on Leibniz and is summarily dismissed in others, it did achieve a certain renown in 1921, when John Maynard Keynes began the preface to his treatise on probability by declaring that "the subject matter of this book was first broached in the brain of Leibniz . . . in the dissertation, written in his twenty-third year, on the mode of electing the kings of Poland."[36]

Leibniz develops his subject in a sequence of numbered propositions, interrupted here and there by the introduction of a corollary or lemma. The content of the individual propositions, however, is not generally mathematical. For example, Proposition 9 reads as follows:

Whatever is contrary to LIBERTY is contrary to SECURITY in Poland.
Whatever is contrary to liberty is contrary to the thing most desired by the Poles, *by prop. 3.*
The Poles are a warlike nation, *by prop. 5.*
Whatever is contrary to the desires of a warlike nation is liable to be a cause of war.
Therefore, whatever is contrary to liberty is liable to be a cause of war in Poland.
Therefore, it is liable to be a cause of civil war.
But civil war is dangerous.
Whatever is dangerous is contrary to security.
Therefore, whatever is contrary to liberty is contrary to security in Poland.

By the time the *Specimen* was published the choice had already been made, and the throne was not given to the candidate for whom Leibniz had argued. Thus the *Specimen* is of interest primarily as a pioneering document in the mathematization of political science.

Throughout his life Leibniz was deeply concerned with aspects of political or social science. His goal was to produce a "general science" (*scientia generalis*) that would embrace mathematics and the physical sciences and the social sciences, using a mathematical method for all of these. He aimed also at a "logique civile" or "logique de la vie" in which practical problems, especially legal questions, would be analyzed by a calculus of probabilities. He wanted in particular to provide an easy and certain way to resolve all disputes. "When controversies arise," he wrote, "there will be no more need for disputation between two philosophers than between two accountants." It "will be enough for them to take their pens in their hands and sit down to their sums and say to each other (calling in a friend if they wish): 'Let us calculate.'"[37]

The realm of seventeenth-century mathematical social science embraces not only the works of thinkers whose aim was to emulate the formal structure of geometric systems or to adopt the abstract certainty of mathematical reasoning but also the attempts to produce a numerical base for understanding society and to propose quantitative analyses. In order to have such social numbers it was necessary to have some kind of census.[38] One example will suffice to indicate the growing feeling of need for census numbers: Sebastian Le Prestre de Vauban (1633–1707), Marshal of France under Louis XIV, who has been described as "France's greatest military engineer." Because of his intense concern for, and great use of, statistical or numerical information Vauban has been called "the father of statistics" or "créateur de la statistique."[39] Fontenelle, in the official *éloge* for the Academy of Sciences, said that Vauban was chosen to be an honorary member of the Academy of Sciences as a mathematician because he, more than any other, "had drawn mathematics down from the skies."[40] He wrote a work on a new system of taxes called the *Dixme* or "Tithe."

The desire to have accurate social numbers or census data was part of the seventeenth-century hope of producing a quantitative science of the state and of society. It was a complement to the stated goal of developing a social science that would resemble mathematics both in form and in the certainty resulting from abstraction, from the absence of discussion of issues and events that would arouse human passion. In order to produce a Galilean social science these mathematical aspects were not sufficient. A technique was needed for producing a mathematical interpretation based on numerical social data. Later in the century an attempt was made to use the new mathematical techniques of algebra and shopkeeper's bookkeeping in order to produce such a social science, a "political arithmetick." These antecedent visions of a non-numerical or non-quantitative mathematical social science were important because they heralded the possibility of transferring to the study of society some of the ideals of mathematics which had proved to be fruitful in the new physical sciences.

4.3. POLITICAL ARITHMETIC AND POLITICAL ANATOMY (GRAUNT AND PETTY)

The very notion of introducing mathematics into the social sciences on the model of the natural sciences today suggests much more than the abstract ideas of Grotius or the geometric form of Leibniz's *Specimen* or Spinoza's *Ethics*. Rather, the term "mathematics" at once invokes both the amassing of numerical or quantitative data and the introduction of mathematical techniques: proportions, algebra, graphs, statistical techniques, the calculus, and other types of higher mathematics.

Although various forms of census and of collecting quantitative data on natural resources and other aspects of the economy long antedated the Scientific Revolution,[41] the first useful series of regularly produced social numbers was the London Bills of Mortality, initially issued, on a weekly basis, early in the sixteenth century. They were discontinued, then reinstituted during plague years and, after 1603, were issued more or less regularly, even during years relatively free of any plague or other widespread disease. At first these Bills gave data only on the number of burials. Then christenings were added. Of even more importance for the statistician was the eventual listing by causes of death other than the plague; then, a separation of burials and christenings according to sex.[42]

An important leap forward in mathematical social science occurred when these data were subjected to analysis by John Graunt (1620–1674), a London draper with little formal education, whose reputation was established by the publication in 1662 of a small book entitled *Natural and Political Observations upon the Bills of Mortality*, which secured him election to the Royal Society, the premier scientific organization in Britain.[43] In the dedication, Graunt observes that his work "depends upon the Mathematicks of my Shop-Arithmetick." That is, Graunt did not make use of academic mathematics such as theoretical geometry or abstract number theory. He used business mathematics or accountancy, adding up totals and subtotals, estimating fractions, and analyzing data in the manner of a businessman. He observed, to take one example, that during a period of twenty years, deaths from "*Small Pox, Swine Pox,* and *Measles*, and of *Worms* without *Convulsions*" totalled 12,210, of which he supposed "about 1/2 might be Children under six years old." Some 16,000 of the total of 229,250 deaths were caused by plague. Hence, "about thirty six *per Century* [i.e., percent] of all quick [i.e., live] conceptions died before six years old." Of this total, "*acute* Diseases" other than the plague accounted for "about 50,000, or 2/9 parts." He concluded that this number gave "a measure of the state, and disposition of this *Climate* and *Air* to health."[44]

Graunt made many analyses of his data, according to such factors as years, seasons, and the regions of London. A whole chapter was devoted to "the difference between the numbers of Males and Females." He essayed an estimate of the number of inhabitants of London and tried to determine the rate of population growth, and he compared "causes of death" in "the Country" and in the city. He posed such general questions as: "What proportion die of such general and particular *Casualties*?" "What Years are Fruitful and Mortal, and in what spaces and Intervals they follow each other?" "Why the *Burials* in *London* exceed the *Christenings*, when the contrary is visible in the *Country*?" Above all he urged that "the Art of Governing, and the true *Politicks*," that is, the science of polity, should be based on quantitative data and their analysis. He concluded that such information was needed about the population (including employment), land, and trade. In short, he urged that statecraft be founded on a quantitative base.[45]

Graunt's pioneering analysis soon bore fruit in the "Political Arithmetick" of Sir William Petty, who had strongly influenced Graunt's work. Petty (1623–1687) led an adventurous life, becoming skilled in mathematics and navigation and eventually obtaining a medical degree from Oxford. While serving as army physician in Ireland, he organized a land survey. On his return to England he became a founding Fellow of the Royal Society. He wrote many tracts on economic subjects, of which the most celebrated is the *Political Arithmetick*, published posthumously in 1690.[46] A preliminary statement declares that Petty invented the name "Political Arithmetick" to denote the way in which "the happiness and greatness of the People, are by the Ordinary Rules of Arithmetic, brought into a sort of Demonstration."[47] Petty set forth his method as follows:

The Method I take to do this, is not yet very usual; for instead of using only comparative and superlative Words, and intellectual Arguments, I have taken the course (as a Specimen of the Political Arithmetick I have long aimed at) to express my self in Terms of *Number, Weight,* or *Measure*; to use only Arguments of Sense, and to consider only such Causes, as have visible Foundations in Nature; leaving those that depend upon the mutable Minds, Opinions, Appetites, and Passions of particular Men, to the Consideration of others. . . .[48]

Like Graunt before him, Petty insists on the primacy of numbers and hence arithmetic and its generalization into algebra.[49] This is the new mathematics, not the traditional geometry of academics which goes back to ancient Greece. Further, the topics with which he is concerned (wealth and trade, shipping, taxes, and the cost of maintaining an army) are dealt with in terms of numerical data. In earlier essays in political arithmetic he studied specific questions of housing, hospitals, and populations. For example, finding that the population of London doubles every forty years and the population of "all England" every 360 years, he concluded that the "Growth of London must stop of its self, before the Year 1800" and that "The World will be fully Peopled within the next Two Thousand Years."[50]

Much as we may admire Petty's boldness in setting forth a program for a polity based on social and economic statistics, we must admit that his effort ended in failure. Among the reasons for his lack of success, the primary one was the insufficiency of accurate numerical data. He was, as he admitted, forced to guess the area of a city. He used the reported number of houses and of burials to estimate the population of London, multiplying the number of burials by thirty and the number of houses by six or sometimes by eight, fully aware that in the absence of a proper census he could produce only approximations. Whenever possible he tried to check his estimates by comparisons with other sources – for instance, asserting that his population estimates "do pretty well agree" with such independent data as the poll-tax returns and the bishops' count of communicants[51] – but he usually did not give the actual numbers and in at least one case, as his modern editor observes, "the agreement between Petty's estimate and the bishops' survey is not strikingly close."[52] That he himself was aware of the deficiencies of his numerical results may be seen in a letter to John Aubrey. "I hope," he wrote, "that no man takes what I say about the living and dyeing of men for a mathematical demonstration."[53]

It must also be noted that Petty often used "rash calculations" and even gave "widely varying estimates for the same things." He was also "frequently inaccurate in his use of authorities" and "careless in his calculations"; on "at least one occasion he is open to suspicion of sophisticating his figures."[54] Petty was severely handicapped by not having the technique of graphs and diagrams for the representation of data. The mathematics he would have needed, probability theory, was just then coming into being. Furthermore, he did not really make any fundamental use of algebraic techniques despite his statements to that effect. While, therefore, we may justly laud Petty's vision and the ideal he set forth, we must also admit that the works he produced did not attain the high standard he proclaimed.

An appreciation of Petty's concern for numbers and mathematics must take account of the fact that he was living in an age when the expanding economy of England and the problems of military statecraft were bringing numerical considerations to the fore. As a result of the research of John Brewer and Keith Thomas, we now have a better understanding of the pressure for numerical information by different departments of state in England in Petty's day. These "constituencies" were, as Brewer has shown, "ministers of the crown," who needed information on "all of the various resources of the different departments in order to exercise firm control over government policy"; the Parliament, "both as a policy-maker and as the body dedicated to securing a responsible executive," which needed government statistics; various "occupational groups and special interests directly affected by state policies," which were "eager to learn the grounds on which such decisions were made"; and even the general public, which had developed "a substantial appetite for the sorts of information that only the very considerable resources of the state could provide."[55]

Petty was trained as a physician and recognized the singular importance of anatomy for medicine. He firmly believed that grounding the new science of polity or statecraft on the mathematical analysis of numerical data was an analogue of basing the study of anatomy on dissection, a practise he had learned while a medical student. His most explicit statement of his politico-anatomical method occurs in a posthumously published work on *The Political Anatomy of Ireland* (London, 1691). In the "Author's Preface," Petty asserts that since anatomy is the only sure foundation for knowledge of the "body natural," it follows that an analogous procedure should be used for the "body politick." To "practice" on the body politic without "knowing the *Symmetry, Fabrick,* and *Proportion* of it," would be to act like uneducated healers, "old-women and Empyricks."[56] Furthermore, "*Anatomy* is not only necessary in Physicians," it is a source of valuable knowledge for "every Philosophical person."[57] Petty proudly declared that he had "attempted *the first Essay of Political Anatomy*."[58]

Petty's introduction presents the political problem in purely anatomical terms: proper "*Dissections* cannot be made without variety of proper Instruments." He is, of course, fully aware of the poor quality and even paucity of statistical data on such matters as land holdings, population, rents, wages, and agricultural production. Even so, he concludes, he has been able "to find whereabouts the Liver and Spleen, and Lungs lye," although he has not been able "to discern" in the state "the Lymphatic Vessels, the *Plexus, Choroidus* [or *Choroides*], the *Volvuli* of vessels within the Testicles." Such statements embody Petty's method of analyzing the functions of the state in the manner of an anatomist performing dissections. He was not primarily seeking for analogues of the functions of the state in human physiology, since his primary goal was not to develop a new fashion of the metaphor of the body politic but rather to create a number-based science of the state and to use the tools of mathematics to disclose the laws and principles of statecraft. Reflecting on his endeavors some three centuries later, we may stand in awe at the majesty of his vision and note that, with the exception of economics – no social science has as yet attained the lofty goal of reducing its fundamental laws and principles to an "arithmetic."

4.4. AN INDEPENDENT "CIVIL" SCIENCE BASED ON MOTION (HOBBES)

Whereas Petty attempted to produce a new science of statecraft by combining numerical analysis with a biomedical approach, Thomas Hobbes (1588–1679)[59] aimed to produce a science of politics or of society based on the new science of motion, concepts of mechanics, and the new physiology.[60] Hobbes was magnificently vain about this achievement. He deserved, he wrote "the reputation of having been the first to lay the grounds of two new sciences": one "of *Optiques*, the most curious, and that other of *Natural Justice*, which I have done in my book *De Cive*, the most profitable of all others."[61] We may note, in passing, that in this passage Hobbes was comparing himself (perhaps unconsciously) with Galileo, whose last great work on the science of motion proudly declared that he had created "two new sciences," as expressed in the title, *Discourses and Demonstration Concerning Two New Sciences* (1638). Hobbes's work in optics has not, in fact, gained him a lasting place among the founders of that subject.[62] But his contributions to political science are universally esteemed and have been the source of many centuries of discussion.

Hobbes boasted of his accomplishment as the founder of a new science of human affairs in another declaration, where he began, in a manner reminiscent of Grotius, with a statement of his high regard for Galileo, whose acquaintance he had sought while in Florence (probably in 1635): "Galileus in our time . . . was the first that opened to us the gate of natural philosophy universal, which is the knowledge of the nature of motion."[63] The presentation of a list of physical scientists led him to biology: "Lastly, the science of man's body, the most profitable part of natural science, was first discovered with admirable sagacity by our countryman Doctor Harvey." And now he assessed his own contribution: "Natural philosophy is therefore but young; but civil philosophy is yet much younger, as being no older . . . than my own book *de Cive*."[64]

In creating a new science of politics Hobbes strove to produce a Galilean social science centering on the concept of motion. He was also influenced by Descartes, notably in his use of the Cartesian concept of a "conatus" or "endeavor" to move and in the adoption of a version of the Cartesian notion of inertia.[65] From both Descartes and Galileo, Hobbes derived his strong belief in the certainty of mathematics. The "great masters of the *mathematics*," he wrote "do not so often err as [do] the great professors of the law."[66] Geometry, he declared in *Leviathan*, "is the only science that it hath pleased God to bestow on mankind."[67] In *De Corpore* he sets forth his mathematical principles and applies them in a somewhat Galilean manner to the analysis of various kinds of motion. But he does so on an abstract level more reminiscent of his medieval predecessors[68] and, for example, does not even refer the Galilean law of uniformly accelerated motion to any physical problem of the observed external or physical world of freely falling bodies. It should be added that our faith in Hobbes as a mathematician is weakened, if not destroyed, by his persistent and unwavering belief that he had been able to square the circle.[69]

Hobbes's political goal has been described as an "attempt to create a philosophic system which embraced the science of natural bodies and extended the methods of that science to human actions and political bodies."[70] He was fully convinced that a science of politics or of human society must be similar to a natural science, based on two primary concepts: movement and matter or substance, in accordance with what was known as the "mechanical philosophy." Transferring the importance of motion from the inorganic to the organic world, Hobbes also drew heavily on the discoveries of William Harvey, which must have had a special significance for him insofar as they were based on mathematics, i.e., on quantitative considerations, and centered on the concept of a continual motion. For Hobbes, the circulation of the blood, the "vital motion" (discovered by "Doctor Harvey"), became the very principle of life, "perpetually circulating," so that the "original of life" was said by him to be "in the heart," which he described as being like a great "piece of machinery in which . . . one wheel gives motion to another."[71]

Thus, Hobbes's political system rejects the traditional organismic metaphor in which the state is considered the analogue of an essentially animate being. Learning from Harvey's physiology, reinforced by the Cartesian philosophy, of the degree to which the animal body functions like a complex mechanical device, Hobbes transformed the old concept of the body politic from a purely animate status to that of a great animal machine, acting like an animal but composed of mechanical parts. Drawing directly on Harvey's comparison of the heart to a pump and of the circulatory system to a hydraulic network of pipes or conduits, Hobbes set forth – on the very first page of the introduction to *Leviathan* – the analogy between a machine and an animal or human body. "The *Heart*," he declares, is nothing "but a *Spring*; and the *Nerves*, but so may *Strings*; and the *Joynts*, but so many *Wheeles*, giving motion to the whole Body." He then compares the state or commonwealth, which is "but an Artificiall Man" to a "Naturall" or biological man. In the detail of the comparison he finds that "*Soveraignty* is an Artificiall *Soul*, . . . giving life and motion to the whole body"; the "*Magistrates*, and other *Officers* of Judicature and Execution" are "artificiall *Joynts*"; "*Reward* and *Punishment*" are "the *Nerves*" (by which "every joynt and member is moved to performe his duty"), and so on. Thus, for Hobbes, the purely organic quality of the traditional analogy has become somewhat lost since the body politic has been transformed into a machine that acts and reacts according to physical rather than biological or vital laws and principles.

Hobbes argued that the use of mathematical (i.e., geometrical) reasoning will produce new exact sciences of the mind and of society, i.e., of ethics and politics. In a tripartite claim he held that "*Reason* is the *pace*; Encrease of *Science*, the *way*, and the Benefit of man-kind, the *end*."[72] Hobbes introduced this subject by comparing reasoning and arithmetic. "When a man *Reasoneth*," he wrote, he does nothing else but "conceive a summe totall, from *Addition* of parcels; or conceive a Remainder, from *Substraction* of one summe from another."[73] But reasoning is also analogous to the methods of demonstration that have traditionally "been used onely in Geometry; whose conclusions have thereby been made indisputable."[74] For Hobbes this method consists

> first in apt imposing of Names; and secondly by getting a good and orderly Method in proceeding from the Elements, which are Names, to Assertions made by Connexion of one of them to another; and so to Syllogismes, which are the Connexions of one Assertion to another, till we come to a knowledge of all the Consequences of names appertaining to the subject in hand; and that is it, men call SCIENCE.[75]

It is to be noted that this mode of procedure was said by Hobbes to lead to predictive rules for a human science and so to produce a guide for obtaining predictable results in the domains of ethics or morals and of political action. In short, Hobbes envisioned a social science that would have some of the same qualities of exactness and of predictability as the physical sciences:

> Science is the knowledge of Consequences, and dependance of one fact upon another: by which, out of what we can presently do, we know how to do something else when we will, or the like, another time: Because when we see how any thing comes about, upon what causes, and by what manner; when the like cause come into our power, we see how to make it produce the like effects.[76]

Hobbes firmly believed that if "the moral philosophers had . . . discharged their duty" as "happily" as "the geometricians have very admirable performed their part," then "I know not what could have been added by human industry to the completion of that happiness, which is consistent with human life."[77] For,

> were the nature of human actions as distinctly known, as the nature of quantity in geometrical figures, the strength of avarice and ambition, which is sustained by the erroneous opinions of the vulgar, as touching the nature of right and wrong, would presently faint and languish; and mankind should enjoy such an immortal peace, that . . . there would hardly be left any pretence for war.[78]

Such was the utopian goal of a social or moral science built by the methods of geometry and natural science.

Hobbes's intellectual debt to Galileo and Harvey, and to Descartes, is apparent in his writings and has been the subject of many commentators.[79] His stress on motion and its laws shows that the philosophy of motion espoused by Galileo and by Descartes had made a deep impression on his thought, even to the belief that "the principles of the politics consist in knowledge of the motions of the mind."[80]

Hobbes later on drew up a comparison of the certainty of geometry and of physics and of "civil philosophy." "Geometry," he wrote, "is demonstrable for the lines and figures from which we reason are drawn and described by ourselves," whereas "Civil philosophy is demonstrable because we make the commonwealth ourselves." But, he argued, "because of natural bodies we know not the construction, but seek it from effects, there lies no demonstration of what the causes be we seek for, but only of what they may be."[81] The science of politics, in short, was less certain than geometry but more certain than physics or natural philosophy.

In the opening sentences of *Leviathan*, Hobbes explained that the state is "an Artificiall Animal," and like "all *Automata*" it has "an artificiall life." Thus it is "by Art" that there "is created that great LEVIATHAN called a COMMON-WEALTH, or STATE (in Latine CIVITAS) which is but an Artificiall Man." Then he presents the structure of the state in terms of analogy with the body; for example, corporations are the muscles, public ministers are the organs or nerves, and the problems of the state are the diseases. These analogies were worked up in some detail. One disease "resembleth the Pleurisie" and yet another "infirmity" is much like that caused by "the little Wormes, which Physicisans call *Ascarides*." Another comparison of the irregularities "of a Commonwealth" and the disease "in the Natural Body of man" focusses on a "Distemper" very much like an "Ague," in which "the fleshy parts being congealed, or by venomous matter obstructed; the Veins which by their naturall course empty themselves into the Heart, are not (as they ought to be) supplyed from the Arteries."[82] This is but one of a number of analogies drawn by Hobbes from the Harveyan circulation of the blood and the functioning of the commonwealth. In another, Hobbes said that money is the blood of the commonwealth, observing that the circulation of money is similar to the circulation of "natural Bloud" which "by circulating, nourisheth by the way, every Member of the Body of Man." There are two movements of money, Hobbes observed, one that conveys it "to the Publique Coffers," the other "that Issueth the same out again for publique payments." In this feature "the Artificall Man maintains his resemblance with the Naturall; whose Veins receiving the Bloud from the several Parts of the Body, carry it to the Heart"; there the blood is "made Vitall" and "the Heart by the Arteries sends it out again, to enliven, and enable for motion all the Members of the same."[83]

It must be kept in mind that in Hobbes's presentation, Leviathan or the commonwealth is not supposed to be an animate natural being, but rather "an Artificiall Man" created by the human mind and endowed by the human artificer with functions analogous to those of a natural person. But even though the commonwealth as "Body Politique" is nothing more than a "fictitious" or "artificall" body, its faculties and properties are known through the study of natural physiology (e.g., the work of Harvey) and its actions are known through the study of natural motions (e.g., the work of Galileo and Descartes plus Hobbes's own innovations). The physiology of Harvey had shown that the heart acts in a manner like that of a mechanical pump, thereby providing Hobbes with evidence that the processes of life might be explained mechanically, just as had been taught by Descartes and other advocates of the "mechanical philosophy." Harvey's work thus gave partial sanction to the likening of the functions of animate beings and machines, even though he had never intended that his research world give sanction to the thesis that all bodily functions of animals and human beings were so mechanical that they could be performed by well designed automata.[84]

Hobbes's achievement was to some degree that he used the new discoveries in physiology to transform the organismic concept of the body politic by giving it a mechanical basis in conformity with Descartes's reductionist philosophy. The political and social world of Hobbes is a hybrid kind of organic structure operating mechanically and conceived under the sign of Galileo, Descartes, and Harvey. His system of society was a collection of human beings acting as "mechanical systems of matter in motion" and, like Grotius before him, he broke away "from the traditional reliance on a supposed will or purpose infusing the universe." Tom Sorell suggests that we misinterpret Hobbes if we assume he was "trying to make the scientific status of physics rub off on his civil philosophy," since Hobbes himself suggested that "he regarded civil philosophy as *more* of a science than physics."[85]

4.5. THE NOTION OF A BALANCE: A SOCIAL SCIENCE BASED ON THE NEW PHYSIOLOGY (HARRINGTON)

Hobbes attempted to introduce some aspects of the life sciences into a system of political thought based primarily on the physical science of motion. But James Harrington (1611–1677) took a quite different tack and, in a conscious rejection of Hobbes's methodology, based a socio-political system squarely on the new Harveyan biology, acting as a "scientist of politics".[86] Harrington's work is all the more significant in that he was "the first English thinker to find the cause of political upheaval in antecedent social change."[87] Furthermore, Harrington was ultimately more influential in the sphere of practical politics than Hobbes – or, for that matter, Vauban, Leibniz, Graunt, or Petty – since his doctrines were implemented in the following century, notably in the form of government adopted in the American Constitution.[88]

During the years of the American Revolution and the Constitutional Conventions, many American statesmen were aware that the concept of "balance" in a socio-political context could be traced to James Harrington's *Oceana*. Thus John Adams wrote in his *Defence of the Constitutions* that this political concept was Harrington's discovery and that he was as much entitled to credit for it as Harvey was for the discovery of the circulation of the blood.[89] In this sentiment Adams was echoing the praise given by John Toland, in his edition of Harrington's works, of which there were two copies in Adams's library.[90]

Harrington's principle of the balance was an expression of his radical position that economic forces influence politics, that political power cannot be considered separately from its economic base. Toland put this idea simply and straightforwardly; it is that "*empire follows the balance of property*, whether lodg'd in one, in a few, or in many hands."[91] To use Harrington's own set of examples: if a king owns or controls three quarters of the land in his realm, there is a balance between his monarchical power and his property. But if the king's property was only one quarter, there would be no balance and any absolute monarchical system would be unstable. Similarly, if "the few or a nobility, or a nobility with the clergy," were the landlords, or should "overbalance the people unto the like proportion," the result would be a "Gothic balance," and "the empire" would be a "mixed monarchy." Finally, there is the case in which "the whole people be landlords, or hold the lands so divided among them, that no one man, or number of men, within the compass of the few or aristocracy, overbalance them." In this event, "the empire (without the interposition of force) is a commonwealth."[92]

In Harrington's interpretation, the crisis of the modern world began for England when, under the Tudors, the power of the feudal nobility was broken and the power of land ownership began to be transferred to the people, thus destroying the more or less stable "Gothic balance." He saw the ultimate effect of this change in the English Civil War and held that "the same impersonal forces" were producing political upheavals on the Continent.

Harrington's ideas are set forth primarily in *The Common-Wealth of Oceana*, which was first published in 1656 and has been described as "a constitutional blueprint" and as "little more than a magnified written constitution."[93] In it he proposed a two-body legislature consisting of an elected "Senate" and a body of elected deputies to be known as "the People." He stressed the use of the ballot and even devised an intricate system of indirect elections which contains features that remind us of the American electoral college. He advocated a strict separation of powers and took a strong position on the need for an explicit written constitution. One of his fundamental principles was the rotation of political offices and a strict limit to the time anyone would be allowed to serve. He was primarily concerned with matters of agrarian policy, advocating a strict upper limit on the amount of land anyone could receive by bequest and an even distribution of family lands. Even so brief a catalogue helps us to understand why *Oceana* influenced many of the statesmen who forged the American system of government.

Harrington was a great admirer of William Harvey and declared that his own work was a "political anatomy," which would make it an analogy of the Harveyan anatomy of the animal body.[94] He firmly believed that his dissection of the problems of his age, together with his remedy in proposing new political institutions, constituted more than the traditional sort of historico-political analysis. According to Harrington, it formed an exact equivalent to the physiological anatomy of William Harvey. The "delivery of a model of government," he wrote, must "embrace all those muscles, nerves, arteries and bones, which are necessary unto any function of a well-ordered commonwealth" and is to be likened to "the admirable structure and great variety of the parts of man's body" as revealed by "anatomists."[95] For Harrington this position implied that the political anatomist, like his physiological counterpart, must base his subject on the principles of nature and not merely on one or two examples. William Harvey, he wrote, did not found his discovery of the circulation of the blood on "the anatomy of this or that body" but rather on "the principles of nature."[96]

Harrington's appreciation of the Harveyan physiology was not limited to generalities, but invoked detailed features of the new biological science. He proposed specific anatomical homologies as well as general analogies. In discussing the two chambers of his proposed legislature, Harrington drew directly on Harvey's *De Motu Cordis*, arguing that "the parliament is the heart," which acts like a suction pump, first sucking in and then pumping out "the life blood of Oceana by a perpetual circulation." In this passage we see Harrington's appreciation of Harvey's radical central idea that the heart is a pump. He even followed Harvey in using the mechanistic language of pump technology, and his concept of a continual process of blood circulation is clearly Harveyan. The mere notion of blood flowing in and out does not require more than a superficial acquaintance with the general aspects of the Harveyan circulation. We have seen that Hobbes used such an analogy with respect to money flowing in and out of the national treasury. But Harrington went much deeper into the physiology of the heart and blood. His statement in full is that "the parliament is the heart which, consisting of two ventricles, the one greater and replenished with a grosser store, the other less and full of a purer, sucketh in and gusheth forth the life blood of Oceana by a perpetual circulation."[97] On close analysis, Harrington's analogy has two aspects that draw the attention of the critical reader. The first is the apparent exclusive concentration on the ventricles, to the exclusion of the auricles; the second is the recognition that there is a physically observable difference between the blood ejected from the left and from the right ventricle, as well as that the ventricles are of unequal size.

The critical reader of this paragraph will note that although Harrington fully appreciated that the ventricles suck in and pump (or gush) out blood, he does not mention that the blood which they expel is sucked in from their respective auricles and not directly from the veins. In this context we should note that Harvey explained the circulation as consisting of two partial cycles. In one, the left ventricle pumps blood out of the heart to pass through the aorta into the main system of arteries, returning to the heart through the venous system, and there entering the right auricle; in the other, sometimes known as the "lesser circulation" (or pulmonary circulation or pulmonary transit), the right ventricle pumps out blood through the pulmonary artery and on into the lungs, to return through the pulmonary vein to the left auricle. Thus the heart produces the circulation by means of two auricles and two ventricles, not by two ventricles alone. Hence the historian must raise the question of whether, when Harrington wrote about a two-chambered rather than a four-chambered heart, he was inadvertently showing that his understanding or knowledge of the Harveyan circulation was imperfect or even superficial. This is not an issue of mere pedantry since it has been alleged that he did not really have a deep understanding of science, even of Harvey's work.[98]

In evaluating Harrington's presentation we must keep in mind that in Harvey's day the auricles were usually considered by physiologists and anatomists to be extensions of the veins leading into the heart, continuations of the inferior and superior vena cava. Thus when Harrington concentrated exclusively on the ventricles, the two chambers that expel or pump out blood from the heart, as the principal chambers of the heart, he simply was not concerned with the auricles, the two chambers by which the blood enters the heart after circulating through the arteries and veins. A similar concentration on two chambers of the heart occurs in Descartes's *Discourse on Method* (1637), one of the early works to recognize the validity of Harvey's discovery. Descartes, who had a sound knowledge of the anatomical structure of the heart, recommended that his readers prepare themselves for reading his discussion by witnessing the dissection of "the heart of some large animal" and by having shown to them "the two chambers [*chambres*] or ventricles [*concavitez*] which are there."[99] Harrington was writing in the style of his time when he ignored the auricles and concentrated on the ventricles.

Harrington's invention of the analogy between the heart and the two chambers of a legislature shows both his knowledge of Harveyan science and his originality. Unlike Hobbes, he took cognizance of Harvey's detailed discussion of the physical difference between the blood pumped out by the left ventricle and that pumped out by the right ventricle. Thus his analogy proposes that the two divisions of the legislature have different functions, just as the blood from the two ventricles has different qualities – "the one greater and replenished with a grosser store; the other less and full of a purer."

Harrington's use of *De Motu Cordis* leaves no doubt concerning his conviction that Harvey's discoveries and method had significant implications for the social scientist. I have found, however, that Harrington's knowledge of Harvey went beyond the circulation and the problems of associated physiology and encompassed other aspects of Harvey's science. Those analyses of Harrington's thought that mention Harvey focus exclusively on Harvey's *De Motu Cordia* and the circulation. But the work on which Harvey spent most of his adult years was his *De Generatione Animalium*, published in 1651. It was in this work that Harvey "created" the "term *epigenesis* in its modern-embryological sense," that is, "to denote the formation of the fetus and of animals by addition of one part after another."[100] The spirit of Harvey's research and conclusions was encapsulated in the allegorical frontispiece, showing Zeus opening a pyxis or egg-shaped box from which a variety of animals spring forth. On the egg there is inscribed boldly *Ex ovo omnia*![101] These words do not occur as such in Harvey's text, but they encapsulate his philosophy of generation, that "all things come from an egg." In a day when scientists tended to believe in some variety of preformation,[102] Harvey championed epigenesis and spent most of his research on an attempt to understand mammalian generation in terms of a fertilized ovum.

Not only was Harrington, as a true student of Harvey, acquainted with the ideas of Harvey's *De Generatione*; he also made use of this aspect of Harvey's science in his political thought. In a posthumously published work entitled *A System of Politics*, Harrington wrote: "Those naturalists that have best written of generation do observe that all things proceed from an egg."[103] Here is a direct English translation of the Latin motto in the frontispiece to Harvey's book. In this essay Harrington showed that he had more than just a general notion of epigenesis; he described aspects of the development of the fetus in the egg of a chick in some detail, following the line of Harvey's discoveries, and he used these facts of embryology as the basis of a political analogy.

Harrington began his presentation of embryology with a discussion of the "punctum saliens," or primordial heart of the chick:

> 1. Those naturalists that have best written of generation do observe that all things proceed from an egg, and that there is in every egg a *punctum saliens*, or a part first moved, as the purple speck observed in those of hens; from the working whereof the other organs or fit members are delineated, distinguished and wrought into one organical body.[104]

The "punctum saliens" had been know to embryologists long before Harvey and was considered the starting point of life, the embryonic heart. Aristotle found, as Harvey recorded, that the "punctum saliens" moved.[105]

Aristotle, and later embryologists, believed that the heart was the first organ to be formed in the development of the chick embryo and that the blood was formed later, after the appearance of the liver. It was a feature of the reigning Galenic physiology in Harvey's day that the blood is manufactured by the liver and so could not exist antecedent to the liver. But Harvey demonstrated by careful experiment that in the chick's egg the blood begins its existence before any organ such as the heart or liver takes form. Harvey's studies showed that in the early stages of development of the hen's egg there appears a little reddish purple point "which is yet so exceedingly small that in its diastole it flashes like the smallest spark of fire, and immediately upon its systole it quite escapes the eye and disappears." This red palpitating (or salient) point, the "punctum saliens," was seen to divide into two parts, pulsating in a reciprocating rhythm, so "that while one is contracted, the other appears shining and swollen with blood" and then, when this one "is shortly after contracted, it straightway discharges the blood that was in it" and so on in a continual reciprocating motion.[106] It has been mentioned earlier that Harvey proudly showed the *punctum saliens* to Charles I. Harvey's conclusions have been summed up as follows. The "blood exists before the pulse" and is "the first part of the embryo which may be said to live"; from the blood "the body of the embryo is made," that is, from it "are formed the blood vessels and the heart, and in due time the liver and the brain."[107] Harrington's paragraph number one summarizes Harvey's embryological findings concerning the "punctum saliens" and the way in which the organs develop from it.

Next Harrington introduces a political analogy. His paragraph number two discusses a "nation without government" or one "fallen into privation of form." It is "like an egg unhatched," Harrington wrote, "and the *punctum saliens*, or first mover from the corruption of the former to the generation of the succeeding form, is either a sole legislator or a council."[108] In paragraph number four, Harrington considers the case of "the *punctum saliens*, or first mover in generation of the form" being "a sole legislator," whose procedure – will be "Not only according to nature, but according to art also," beginning "with the delineation of distinct orders of members." This "delineation of distinct organs or members (as to the form of government)," Harrington continues in paragraph number five, is "a division of the territory into fit precincts once stated for all, and a formation of them to their proper offices and functions, according to the nature or truth of the form to be introduced."[109]

In his paragraph number four, Harrington makes a distinction between analysis of the political state by proceeding "according to nature" and "according to art." Again and again in his various writings, he introduced this difference between fundamental science (knowledge of nature) and art (the applications of scientific knowledge). He held that in natural science as well as in political science or the science of society, principles may exist in nature which have not yet been discovered (by science) or applied (in an art). The concept or principle of the political balance, he pointed out, was "as ancient in nature as herself, and yet as new in Art as my writings."[110] Harrington contrasted the originality of his discovery and the eternal nature of the principle he had discovered by comparing his example with that of Harvey. The occasion was a disparagement of *Oceana* by Matthew Wren, who had argued that the principle of the balance was not at all an extraordinary or new discovery made by Harrington but was only a restatement of what had always in some sense be known. Harrington replied by saying that the situation was as if one were to tell "Dr. Harvey that . . . he had given the world cause to complain of a great disappointment in not showing a man to be made of gingerbread, and his veins to run malmesey,"[111] rather than pointing out characteristics of blood known since time immemorial.

Harvey's *De Generatione Animalium* would have been of signal importance for Harrington because of one feature in which it differs markedly from *De Motu Cordis. De Motu Cordis* contains passing remarks on method here and there, but *De Generatione Animalium* presents a general discussion of method – how to do science or how to reason correctly in studying nature – in several short essays that form a preface to the whole work. Here Harvey expressly states that his aim is not merely to make known the new information he has acquired about generation "but also, and this in particular," to "set before studious men a new and, if I mistake not, a surer path to the attainment of knowledge." That way is to study nature and not books, to follow "Nature's lead with their own eyes":

> Nature herself must be our adviser; the path she chalks must be our walk, for thus while we confer with our own eyes and take our rise from meaner things to higher, we shall at length be received into her closet-secrets.[112]

A thinker like Harrington could well have imagined that Harvey was speaking directly to him.

Harvey's methodological essays have two very different aspects which, to a twentieth-century reader, may seem contradictory at first encounter. For not only did Harvey establish certain rules for the direction of research and for producing new knowledge; he also sought to establish a kinship with Aristotle as the master of experimental biological science and first formulator of the mode of biological investigation, the philosopher who had stressed particularly the roles of sensory perception and memory and the path from singulars to universals. It is one of the paradoxes of the history of thought that Harvey should have revolutionized biology while being, to so considerably a degree, an Aristotelian. But even though Harvey constantly praised Aristotle, he did not as a result cease to call attention to Aristotle's mistakes and to correct them.

Again and again in *De Generatione Animalium*, and especially in the methodological essays, Harvey insisted that experience – i.e., direct experiment and observation – is the only way to learn about nature. Thus "sense and experience" are " . . . the source of both . . . Art and Knowledge." Further, he declared, "in every discipline, diligent observation is . . . a prerequisite, and the senses themselves must frequently be consulted." He even went to the extreme of imploring his readers to "take on trust nothing that I say" and calling upon their eyes to be my "witnesses and judges."[113]

The study of nature requires, according to Harvey, not only diligent but repeated observations. For Harvey, observations of nature have two separate aspects. The first is to make a careful description and delineation of each organ or part of the animal or human body; the second to perform what we would call experiments, but which in Harvey's day had not yet been separated out from the more general term "experience" (as is still the case for French and Italian). Harvey's contemporary Kenelm Digby had this latter feature of Harvey's work in mind when he wrote that "Dr. Harvey findeth by experience and teacheth how to make this experience."[114] Stressing the method of induction, Harvey never discussed the great leap of imagination that produces hypotheses to be tested nor did he discuss the directive ideas that are of such great importance in any research program.[115]

Harvey's research program was always guided by his conception of the purpose of anatomical study, displaying one further component that would have been of significance for Harrington. Harvey saw the goal of the anatomist to be a study of the parts of the body in order to determine structures, so that knowledge of such structures could lead to an understanding of functions. By the end of the seventeenth century, this latter aspect of the subject, on which Harvey had left a strong impress, had become generally recognized. That broad compendium of seventeenth-century science, John Harris's *Lexicon Technicum* (1704), defines anatomy as "an Artifical Dissection of an Animal, especially Man," in which "the Parts are severally discovered and explained" in order to serve "for the use of Physick and Natural Philosophy." The new anatomy, in which Harvey was a leading figure, has been characterized as a transformation of the old descriptive or "dead" anatomy into an "anatomia animata," a change from the static "description and drawing up of an inventory of the parts of the human body" to a dynamic understanding of the functions of each part in its structure and of each structure in the processes of life. In short, Harvey proposed what we would call today a study of anatomy in order to produce a physiology.[116]

The political anatomist, following Harvey's precepts, would thus seek detailed information to be organized into political structures – information based on direct experience. William Petty thought that statistical data might replace dissection as a source of experiential information, but Harrington – following the precepts and example of Harvey – held that direct observation was required in the political realm. Harrington's method was like that of the empirical scientist or anatomist, exemplified by Harvey. Experience for the anatomist is the study of actual bodies, living and dead, whereas for the scientist of politics the sources of experience are personal contact (by travel) and reading in the records of history. "No man can be a politician [political scientist]," Harrington wrote, "except he be first an historian or a traveller; for except he can see what must be, or may be, he is no politician." If a man "hath no knowledge in history, he cannot tell what hath been," Harrington pointed out, "and if he hath not been a traveller, he cannot tell what is: but he that neither knoweth what hath been, nor what is, can never tell what must be or what may be."[117]

Harrington's "comparison of the study of politics with anatomy," to quote Charles Blitzer, "was not simply a casual simile," but rather "represented a reasoned belief in the basic likeness of the two disciplines." Both the body politic and the human body are composed of similarly interlocking machine-like structures which function in a co-ordinated manner. Harvey had achieved great success in studying the human body by the method of experience and reason; surely the political anatomist might hope for similar results of significance from the application of a similar method.

In a reply to Matthew Wren, Harrington reminded his critic that "anatomy is an art; but he that demonstrates by this art demonstrates by nature."[118] So is it "in the politics," he wrote, "which are not to be erected upon fancy, but upon the known course of nature," just as anatomy "is not to be contradicted by fancy but by demonstration out of nature." It is "no otherwise in the politics," he concluded, than in anatomy.[119] In short, the study of politics and the study of anatomy were alike because they both sought principles of nature by reason and experience. But Harrington would have agreed with Harvey that one should not be subservient to "the authority of the Ancients." For Harvey the rule was that "the deeds of nature . . . care not for any opinion or any antiquity," that "there is nothing more antient then nature, or of greater authority." Harrington agreed. In the "Epistle Dedicatory," of *De Motu Cordis*, Harvey explained that he did not "profess either to learn or to teach anatomy from books or from the maxims of philosophers" (i.e., from the "works" and "opinions of authors and anatomical writers"), but rather "from dissections and from the fabric of Nature herself."[120] In his second reply to his critics, as represented by Jean Riolan, he referred to confirmation of his ideas by "experiments, observations, and ocular testimony." Harrington, in effect, translated these precepts from human to political anatomy.

Harrington made a deliberate choice in adopting for political science the method of the anatomist, with reliance on direct observation and "experience." That is, he consciously rejected the path of the physical sciences and mathematics. He pilloried Hobbes's use of mathematics in a political context because he particularly abhorred deductive systems that emulated geometry, of which he found a primary example in Hobbes's "ratiocination." Again and again he openly expressed his scorn for what he sometimes called "geometry," sometimes "mathematics," and sometimes "natural philosophy."[121] He made fun of Hobbes for supposing that one could establish a monarchy "by geometry."[122]

Harrington also disdained the physical sciences as a source of models or analogies for politics. He believed that physical science tends to produce abstractions rather than actualities. On this point his views were in harmony with Harvey's. "The knowledge we have of the heavenly bodies," Harvey wrote in the second letter to Jean Riolan, is "uncertain and conjectural." No doubt he had in mind the impossibility of proving whether the Copernican or Ptolemaic or Tychonic system (or any other possible variant) is the true one.[123] In any case, he left no doubt concerning his position: "The example of Astronomie is not here to be followed." The reason, he explained, is that astronomers argue indirectly from observed phenomena to "the causes" and to the reason "why such a thing should be." But for astronomers to proceed as anatomists do in this research, they would have to seek "the cause of the Eclipse" by using direct sense observation, and not by relying on reason alone, and thus would have to be situated beyond the moon.[124]

This astronomical uncertainty can be easily contrasted with the clarity and definiteness of Harvey's proofs. In *De Motu Cordis*, Harvey assembles, one by one, the elements of direct evidence that the heart is pumping blood and that there is a correlation between the systole and diastole of the pulse and the contractions and dilations of the heart, that the blood flows from the heart outward through the arteries and inward toward the heart through the veins, that the valves in the veins permit the passage of blood only in a direction toward the heart. Faced with such anatomical certainty, especially as contrasted with astronomical uncertainty, Harrington made the obvious choice of a scientific model for his political thought.[125]

Harrington's political anatomy resembled Harvey's human and animal anatomy insofar as the purpose was to produce an accurate and careful description and delineation of parts or of anatomical features as a guide to function. Both Harvey and Harrington studied structures with the ultimate goal of producing a general synthesis, in which the working of every structure or organ would become known in relation to its form and structure, so that its actions could be understood as part of the functioning of the body as a living whole. A similar purpose inspirits the anatomical-physiological study of both the animal or human body and the body politic.

Harrington is of special interest in the present context because his political ideas were associated with the science of his day, but especially because his ideas eventually influenced practical policy. His writings and those of Hobbes were among the most widely discussed works of the seventeenth century that embodied the application of science to the socio-political arena. Both *Leviathan* and *Oceana* made extensive use of the new life science. Hobbes combined the organismic analogy with principles of physics and the methods and ideals of mathematics, but Harrington expressly denied that mathematics and mathematical physical science could provide a key to politics. As we have seen, Harrington believed that the principles of politics would be revealed by political anatomy, that is, by following the empirical method of Harvey's anatomy-based life science which yielded principles learned from what Harvey called "the fabric of Nature."[126] Hobbes aimed to transform the centuries-old organismic concept of the body politic into a new metaphor embodying the notion of mechanical system. But Harrington endowed this ancient concept with the qualities of the new Harveyan physiology and so developed a revised biological metaphor and a political science that embodied the chief features of the "Scientific Revolution."

4.6. CONCLUSION

Although the early seventeenth century withnessed a number of attempts to construct one or another social science on principles of mathematics or the natural sciences, no social science of this era ever reached the high level of achievement of Galileo's physics of motion or Harvey's physiology. From the historical perspective these attempts – in the early decades of the Scientific Revolution – to produce a social science that would be equaivalent to the natural sciences are of interest primarily in exhibiting the efforts of many thinkers to understand society and its institutions with the same success that natural scientists achieved with respect to the world of observed nature. In truth, any judgment on the results of these seventeenth-century precursors must take into account that today most social sciences do not exactly resemble their counterpart natural sciences.

Why did these early social scientists seek in the natural sciences a model for their own subjects or attempt to create a social science along the lines of the natural sciences? First of all, there was a natural desire to emulate the works of a Galileo or a Harvey and to share in the accolades given to the natural sciences by using their methods and metpahors in the social domain. Additionally, there was a consensus that the soundest way to create a new science of society was to jettison the traditional reliance on established authorities, such as Plato, Aristotle and the scholastic "doctors," and to start afresh with the new source of authority, nature "herself." Those who practised the natural sciences held that the supreme authority was lodged in nature, and not in the writings of ancient and medieval sages. Seeking an equivalent of the experientially revealed world of nature, social scientists turned to travel or political and social data and to the records of history.

In my analysis I have concentrated on the actual use of the natural sciences by those who aimed to create a social science based upon them. While reading the works of these thinkers, I was impressed again and again by the profundity of their conviction that the natural sciences hold the key to the creation of a science of human behavior and human institutions. Their statements, as I have quoted and summarized them, are strong and unambiguous. Yet it must be admitted that the portions of their works with which I have dealt account for only a small part of the total oeuvre of these writers.[127] In terms of space, and even of direct prominence of presentation, the matters with which I have been concerned may in fact be only a very small part of the treatises published by most of my group of social scientists. Furthermore, it is a fact that today's encyclopedias of the social sciences and general works on the history of political and social theory do not generally mention that Grotius and Harrington, by their own testimony, declared the dependence of their social and political thought on mathematics and the Galilean physics of motion or on the Harveyan physiology. As mentioned, Leibniz's political essay is not cited in general histories of political thought or in even most works on Leibniz. Even Hobbes's use of Galilean physics and Harveyan physiology is discussed only in specialized works. And thus we are lead at once to a pair of fundamental questions. One is whether the use of the natural sciences was really an integral part of their thought as I have alleged, or whether the introduction of the natural sciences was merely a variety of rhetorical flourish so characteristic of that age. The second is why was it that such works as *De Jure Belli ac Pacis* or *Oceana* were not so permeated with the new science that readers could not help but be constantly aware of the foundation in the natural sciences that has been discerned only through detailed historical research?

The second question can be answered more easily and helps us to deal with the first one. If these treatises had been written in such a way that no significant body of the text could be read without some knowledge or understanding of the new mathematics or the natural sciences, then the number of potential readers would have been greatly restricted. Only scholars who were interested in the social sciences and had a scientific or mathematical preparation would have proved equal to the task. Thus, the influence of the works would have been limited. Newton faced a similar situation when he wrote his *Principia*. If he had recast every argument and proof in terms of the algorithm of the calculus which he had invented, the only readers would be those few who had both mastered the new mathematics and were able and willing to adopt a new rational mechanics. On the other hand, if he proceeded in a more geometric and somewhat traditional manner, introducing algebraic formulations of the calculus here and there, he would not frighten away potential readers by facing them with unnecessary chevaux de frise.

The conclusion to which we are led is that the absolute quantity or degree of ubiquity of mathematics or of natural science in the early treatises on the social sciences can not be taken as an index of the degree to which the authors conceived a deep inner dependence on mathematics or on the natural sciences. From today's retrospection what is most significant, therefore, is not the number and extent of the discussions directly involving the natural sciences in the works on political or social science of the seventeenth century, but rather the fact that there are such passages at all. It must have required courage and foresight to attempt to enlarge the domain of the natural sciences by applying the methods of those disciplines to the complex problems of society and of human institutions.

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NOTES

1 Throughout this chapter, I use the term "social science" anachronistically to designate a science of any organized aspect of society. This rubric therefore includes thoughts about society in terms of organization or improvement, international law, statecraft and civil polity, theories of government or the state, and so on. The term "social science" did not come into being until late in the eighteenth century and, as is well known, "sociology" was invented by Comte in the early nineteenth century. See, further, Appendix on Social Science at the end of this volume.

2 In using the word "science" in the discourse of the seventeenth and eighteenth centuries, we must remember that this term did not exclusively designate the area of the natural sciences or mathematics but could be used for any organized branch of knowledge. See §1.1 supra.

3 Some of the scientists who hoped for a second "Newton" were such diverse specialists as the anatomist and paleontologist Baron Georges Cuvier and the physical chemists Otto Heinrich Warburg, Jacobus Henricus van't Hoff, and Friedrich Wilhelm Ostwald; see

I.B. Cohen: *The Newtonian Revolution* (Cambridge/New York: Cambridge University Press, 1980), p. 294.

4 See my discussion of "Newton and the Social Sciences: The Case of the Missing Paradigm," to appear in Philip Mirowski (ed.): *Markets Read in Tooth and Claw* (Cambridge/New York: Cambridge University Press, 1993) [in press].

5 In producing this list I gladly acknowledge the influence of Alexander Koyré. See his *Etudes galiléennes* (Paris: Hermann, 1939); trans. John Mapham as *Galilean Studies* (London: Harvester Press; Atlantic Highlands [N. J.]: Humanities Press, 1978). Also, Koyré's *Metaphysics and Measurement: Essays in Scientific Revolution* (Cambridge: Harvard University Press, 1968). H. Floris Cohen has made a study of the different major interpretations of the causes of the Scientific Revolution; his book on this subject (titled *The Banquet of Truth*) is currently being readied by publication.

6 The telescope showed that Venus exhibits a sequence of phases which could not occur in the Ptolemaic system. See I.B. Cohen: *The Birth of a New Physics* (revised ed., New York: W.W. Norton & Company, 1985), ch. 4.

7 The famous experiment of dropping uneven weights from a tower could prove the falsity of the doctrine that heavy bodies fall with speeds proportional to their weights, but could not reveal the laws of motion.

8 The possible experimental basis of Galileo's discovery of the laws of motion remained secret (that is, confined to Galileo's manuscripts) until our own times, when Stillman Drake began to study the unpublished manuscripts.

9 Galileo's final presentation of the laws of motion appears in his *Discourses and Demonstrations Concerning Two New Sciences* (1642). In this work, general discussions are in Italian, the language suited for a dialogue in prose, while the mathematical demonstrations are in Latin, and thus set apart from the discussion of general principles.

10 I have called this method the "Newtonian style," since it was brought to fulfillment and used most effectively by Newton, even thought its roots can be traced back to Galileo. On this subject see the work cited in n. 3 supra and also §1.4 supra.

11 Although Descartes's contributions to mathematics are presented in every history of the subject, there has never been until recently a full-length and adequate study of Descartes as a physicist. See William R. Shea: *The Magic of Numbers and Motion: The Scientific Career of René Descartes* (Canton [Mass.]: Science History Publications, 1991). On Descartes and the science of motion see René Dugas: *Histoire de la Mécanique* (Neuchâtel: Editions du Griffon, 1950), trans. J.R. Maddox as *A History of Mechanics* (Neuchâtel: Editions du Griffon; New York: Central Book Company, 1955). Also R. Dugas: *La mécanique au XVIIe siécle: des antécédents scolastiques à la pensée classique* (Neuchâtel: Editions du Griffon, 1954), trans. Freda Jacquot as *Mechanics in the Seventeenth Century: From the Scholastic Antecedents to Classical Thought* (Neuchâtel: Editions du Griffon; New York: Central Book Company, 1958).

12 Descartes expressed this belief in a letter of 15 June 1646 to Pierre Chanut, the French ambassador to Sweden and brother-in-law of Claude Clerselier, the translator of Descartes's works into French and editor of the first collection of Descartes's letters. In this letter, Descartes explained how his "knowledge of physics" has been "a great help to me in establishing sure foundations in moral philosophy." He declared that he had "found it easier to reach satisfactory conclusions on this topic than on many others concerning medicine on which I have spent much more time." Accordingly, "instead of finding

ways to preserve life," he had "found another, much easier and surer way, which is not to fear death." Quoted from Descartes's *Philosophical Letters*, trans. Anthony Kenny (Oxford: Clarendon Press, 1970; Minneapolis: University of Minnesota Press, 1981), p. 196. On Descartes's physiology, see his *Treatise of Man*, trans. Thomas Steele Hall, with introduction and commentary (Cambridge: Harvard University Press, 1972).

[13] William Harvey: *An Anatomical Disputation concerning the Movement of the Heart and Blood in Living Creatures*, trans. Gweneth Whitteridge (Oxford/London: Blackwell Scientific Publications, 1976), p. 75; see also *The Anatomical Exercises of Dr. William Harvey: De Motu Cordis, 1628; De Circulatione Sanguinis, 1649: the First English Text of 1653*, ed. Geoffrey Keynes (London: The Nonesuch Press, 1928), reprinted (without "The Circulation of the Blood") in William Harvey: *Exercitatio Anatomica de Motu Cordis et Sanguinis in Animalibus: Being a Facsimile of the 1628 Francofurti Edition, Together with the Keynes English Translation of 1928* (Birmingham: The Classics of Medicine Library, 1978), p. 58; also "An Anatomical Disquisition on the Motion of the Heart and Blood in Animals," trans. Robert Willis (n. 105 infra), p. 46; also *Movement of the Heart and Blood in Animals: An Anatomical Essay by William Harvey*, trans. Kenneth J. Franklin (Oxford: Blackwell Scientific Publications, 1957), p. 58. These are cited as Whitteridge trans., Willis trans., and Keynes.

On the role of quantitative considerations in the genesis of Harvey's discovery of the circulation, see §2 of the introduction to the Whitteridge translation; also Gweneth Whitteridge: *William Harvey and the Circulation of the Blood* (London: Macdonald; New York: American Elsevier, 1971 – cited as Whitteridge). Also Frederick G. Kilgour: "William Harvey's Use of the Quantitative Method," *Yale Journal of Biology and Medicine*, 1954, **26**: 410–421.

[14] Cf. Keynes 1928 (n. 13 supra), pp. vii–viii; Keynes 1978 (n. 13 supra), pp. v–vi; see also Whitteridge trans. (n. 13 supra), p. 3; Willis trans. (n. 13 supra), pp. 3–4; Franklin trans. (n. 13 supra), p. 3. The Latin text of 1628 is reprinted in facsimile as the first half of Keynes 1978, pp. 3–4. In quoting this passage I use a combined version including some corrections introduced from the original Latin and inclining towards the English translation of 1653, the text which, together with the Latin, would have been available to readers, such as James Harrington, in the seventeenth century.

[15] Whitteridge trans. (n. 105 infra), p. 359; also Willis trans. (n. 105 infra), p. 485. On the significance of the "punctum saliens" in a political context, see §4.5 infra.

[16] Whitteridge (n. 13 supra), pp. 214, 235. Harvey's own description of this episode is given in his *De Generatione Animalium*, Whitteridge trans. (n. 105 infra), pp. 249–251; also Willis trans. (n. 105 infra), pp. 382–384.

[17] On the body politic, see David George Hale: *The Body Politic: A Political Metaphor in Renaissance Literature* (The Hague/Paris: Mouton, 1971), a valuable study even though Hale never considers the relation of the socio-political concept of the body politic to the reigning physiological theories of the body's functioning.

[18] From "The Prologue to the Reader," in John Halle (compiler): *A Very Frutefull and Necessary Briefe Worke of Anatomie, or Dissection of the Body of Man . . . , with a commodious order of notes, leading the chirurgien's hande from all offence and error . . . compiled in three treatises* (London: Thomas Marshe, 1565), published as part of *A Most Excellent and Learned Worke of Chirurgerie, called Chirurgia parva Lanfranchi* . . . (London: Thomas Marshe, 1565).

[19] On Harvey's attitude towards the liver, see Whitteridge (n. 13 supra), esp. p. 142. On the difference between the status assigned to the heart and to the blood by Harvey in *De Generatione* and in *De Motu Cordis*, see n. 21 infra.

[20] Whitteridge trans. (n. 105 infra), p. 242; see also Willis trans. (n. 105 infra) pp. 374–375.

[21] Whitteridge trans. (n. 13 supra), pp. 120, 129–30. In *De Motu Cordis*, Harvey was almost exclusively concerned with the function of the heart as the primary agent producing the circulation and not with the question of whether the heart comes into being in the embryo before the blood. In various other works, and notably in the *De Generatione Animalium*, Harvey made it plain that the blood appears in the development of the embryo before the heart or the liver or any other organ. On Harvey's views concerning the status of the heart and of the blood, especially the difference between *De Generatione* and *De Motu Cordis* and between Harvey's and Aristotle's positions on this topic, see Whitteridge (n. 13 supra), pp. 215–235, and §4.5 infra.

This issue is debated in a set of three articles in *Past and Present*: an original presentation of "William Harvey and the Idea of Monarchy" by Christopher Hill (no. 27, April 1964), a rebuttal by Gweneth Whitteridge (no. 30, April 1965: "William Harvey: A Royalist and No Parliamentarian"), and a reply by Hill (no. 31, July 1965: "William Harvey (No Parliamentarian, No Heretic) and the Idea of Monarchy." These articles are reprinted in Charles Webster: *The Intellectual Revolution of the Seventeenth Century* (London/Boston: Routledge & Kegan Paul, 1974), pp. 160–181, 182–188, 189–196.

Hill's final disclaimer undermines his statement (p. 112) that Harvey's later views have implications which "can only be described as republican – or at best they suggest a monarchy based on popular consent." There is no evidence that Harvey changed his political position from staunch Royalist to supporter of the Commonwealth.

[22] Jacob ter Meulen and P.J.J. Diermanse: *Bibliographie des écrits imprimés de Hugo Grotius* (The Hague: Martinus Nijhoff, 1950), no. 407; Christian Gellinek: *Hugo Grotius* (Boston: Twayne Publishers, 1983), pp. 40, 128 n.78; Hamilton Vreeland: *Hugo Grotius: The Father of the Modern Science of International Law* (New York: Oxford University Press, 1917; reprint, Littleton, Colorado: Fred B. Rothman & Co., 1986), p. 29; M.G.J. Minnaert: "Stevin, Simon," *Dictionary of Scientific Biography*, vol. 13 (New York: Charles Scribner's Sons, 1976), p. 49; Ben Vermeulen: "Simon Stevin and the Geometrical Method in *De Jure Praedae*," *Grotiana*, 1983, **4**: 63–66. Dirk J. Struik: *The Land of Stevin and Huygens: A Sketch of Science and Technology in the Dutch Republic during the Golden Century* (Dordrecht/Boston/London: D. Reidel Publishing Company, 1981), pp. 47, 53, 56.

On Grotius's life and career, see William S.M. Knight: *The Life and Works of Hugo Grotius* (Reading: The Eastern Press, 1925). See also E.H. Kossmann: "Grotius, Hugo," *International Encyclopedia of the Social Sciences*, vol. 6 (New York: The Macmillan Company & The Free Press, 1968); *The World of Hugo Grotius (1583–1645)*: Proceedings of the International Colloquium Organized by the Grotius Committee of the Royal Netherlands Academy of Arts and Sciences, Rotterdam, 6–9 April 1983 (Amsterdam & Maarsen: APA-Holland University Press, 1984); Stephen Buckle: *Natural Law and the Theory of Property: Grotius to Hume* (Oxford: Clarendon Press, 1991); Hedley Bull, Benedict Kingsbury, and Adam Roberts (eds.): *Hugo Grotius and International Relations*

(Oxford: Clarendon Press, 1990); Edward Dumbauld: *The Life and Legal Writings of Hugo Grotius* (Norman: University of Oklahoma Press, 1969); Charles S. Edwards: *Hugo Grotius: The Miracle of Holland: A Study in Political and Legal Thought* (Chicago: Nelson-Hall, 1981).

The Carnegie Endowment for International Peace has published a good translation by Francis W. Kelsey of *De Jure Belli ac Pacis Libri Tres* (Oxford: Clarendon Press; London: Humphrey Milford, 1925 – The Classics of International Law, no. 3, vol. 2); in the same series (no. 3, vol. 1) is a facsimile reproduction of the Latin edition of 1646 (Washington: Carnegie Institution of Washington, 1913). See also Hugo Grotius: *De Jure Belli ac Pacis Libri Tres*, ed. and trans. William Whewell, 3 vols. (Cambridge: John W. Parker, London, 1853). In this edition, the English translation (an abridged version) appears at the bottom of the page underneath the Latin text.

[23] Galileo Galilei: *Le Opere*, vol. 16 (Florence: Tipografia Barbèra, 1905 and later reprints), pp. 488–489, a letter from Hugo Grotius in Paris to Galileo, written in September 1636; also in Hugo Grotius: *Briefwisseling*, vol. 7, ed. B.L. Meulenbroek (The Hague: Martinus Nijhoff, 1969 – Rijks Geschiedkundige Publicatiën, Grote Series, 130), pp. 398–399.

Grotius wanted to find an asylum for Galileo when the latter had been condemned by the Inquisition. See Hugo Grotius: *Briefwisseling*, vol. 5, ed. B.L. Meulenbroek, (The Hague: Martinus Nijhoff, 1966 – Rijks Geschiedkundige Publicatien Grote Serie 119), pp. 489–490. See also Giorgio de Santillana: *The Crime of Galileo* (Chicago/London: The University of Chicago Press, 1955; Midway reprint, 1976), p. 214 n. 17.

[24] Kelsey trans. (n. 22 supra), pp. 23, 29–30; also Whewell trans. (n. 22 supra), vol. 1, pp. lxv, lxxvii.

[25] Hugo Grotius: *De Jure Praedae Commentarius: Commentary on the Law of Prize and Booty*, vol. 1: A Translation of the Original Manuscript of 1604 by Gladys L. Williams with the collaboration of Walter H. Zeydel (Oxford: at the Clarendon Press; London: Geoffrey Cumberlege, 1950 – Publications of the Carnegie Endowment for International Peace, Washington; The Classics of International Law, no. 2, vol. 1; also reprinted, New York: Oceana Publications; London: Wiley & Sons, 1964), p. 7; Hugo Grotius: *De Jure Praedae Commentarius*, vol. 2: The Collotype Reproduction of the Original Manuscript of 1604 in the Handwriting of Grotius (Oxford: at the Clarendon Press; London: Geoffrey Cumberlege, 1950 – Publications of the Carnegie Endowment for International Peace, Washington; The Classics of International Law, no. 2, vol. 2), f. 5^r; Ben Vermeulen (n. 22 supra), p. 63 (with specific mention of his not discussing "the non-juridical chapters XIV and XV); cf. also Alfred Dufour: "L'influence de la méthodologie des sciences physiques et mathématiques sur les fondateurs de l'Ecole du Droit naturel moderne (Grotius, Hobbes, Pufendorf)," *Grotiana*, 1980, 1: 33–52, esp. 40–44; Alfred Dufour: "Grotius e le droit naturel du dix-septième siècle," in *The World of Hugo Grotius* (n. 22 supra), pp. 15–41, esp. 22–23; Peter Haggenmocher: "Grotius and Gentili: A Reassessment of Thomas E. Holland's Inaugural Lecture," in Bull (n. 22 supra), pp. 142–144, 162; C.G. Roelofsen, "Grotius and the International Politics of the Seventeenth Century," in Bull, pp. 99, 103–111. It must also be said that the mathematical aspect should not be overemphasized; Knight (n. 22 supra), for example, thinks of the procedure in *De Jure Praede* as scholastic (p. 84). The revised twelfth chapter of *De Jure Praedae* was published in 1609 as *Mare Liberum*. The manuscript of *De Jure Praedae* was dis-

covered in 1864 and finally published in full as *De Jure Praedae Commentarius*, ed. H.G. Hamaker (The Hague: Martinus Nijhoff, 1868). See Meulen and Diermause (n. 22 supra), nos. 541, 684. It should be noted that the geometrical form of *De Jure Praedae* is much less striking than that of Leibniz in his *Specimen* (n. 36 infra). The two documents are comparable, however, because of their invocation and use of mathematical method, their addressing of a specific contemporary crisis, and the youth of their authors.

[26] Kelsey trans. (n. 22 supra), p. 29; also Whewell trans. (n. 22 supra), vol. 1, p. lxxvii. Voisé (n. 30 infra), p. 86.

[27] Kelsey trans. (n. 22 supra), pp. 40, 13; Whewell trans. (n. 22 supra), vol. 1, pp. 12, xliv–xlvi. See also Ernst Cassirer: *The Myth of the State* (New Haven: Yale University Press, 1946), p. 172; reprint (Garden City, N.Y.: Doubleday & Company [Doubleday Anchor Books], 1955), p. 216; also, e.g., Hendrik van Eikema Hommes: "Grotius on Natural and International Law," *Netherlands International Law Review*, 1983, **30**: 61–71, esp. 67.

[28] Voisé (n. 30 infra), p. 86. Cf. Jerzy Lande, *Studia z filozofii prawa*, ed. Kazimierz Opalek & Jerzy Wróblewski (Warsaw: Panstwowe Wydawnictwo Naukowe, 1959), pp. 537–543.

[29] Johan Huizinga: *Men and Ideas: History, the Middle Ages, the Renaissance,* trans. James S. Holmes and Hans van Marle (New York: Meridian Books, 1959), pp. 332–333, 337–338; and Voisé (n. 30 infra), p. 85.

[30] Hugo Grotius: *The Rights of War and Peace*, trans. A.C. Campbell (Washington/London: M. Walter Dunne, 1901; reprint, Westport, Conn.: Hyperion Press, 1979).

Cassirer (n. 27 supra, p. 165), of course, was aware of Grotius's admiration for Galileo and Grotius's reliance on the method of mathematics, but even he did not deal in full with these topics. The only work which I have encountered which seriously addresses this aspect of Grotius's career is Waldemar Voisé: *La réflexion présociologique d'Erasme à Montesquieu* (Wroclaw: Zaklad Narodowy Imienia Ossolinskich, Wydawnictwo Polskiej Akademii Nauk, 1977), esp. pp. 84–87. But even Voisé does not explore fully the consequences of Grotius's choice of a mathematical model.

[31] Voisé (n. 30 supra), p. 88.

[32] Ibid., pp. 88–89.

[33] Spinoza's *Ethics*, published posthumously, is available in a number of different English editions. A good, recent reference work on Spinoza's *Ethics* is Jonathan Bennett: *A Study of Spinoza's Ethics* (Indianapolis: Hackett Publishing, 1984). Spinoza's work on Descartes's *Principles of Philosophy* was translated by Halbert Hains Britan (Chicago: The Open Court, 1905).

[34] Benedict Spinoza: *The Political Works*, ed. and trans. A.G. Wernham (Oxford: Clarendon Press, 1958), p. 263. This volume contains a very valuable historical and critical study plus the complete text of the *Tractatus Politicus* and a translation of the major portions of the *Tractatus Theologico-Politicus.*

[35] Idem.

[36] See John Maynard Keynes: *A Treatise on Probability* (London: Macmillan and Co., 1921; reprint, New York: AMS Press, 1979), p.v.; also reprinted as vol. 8 of *The Collected Writings of John Maynard Keynes* (London: Macmillan for the Royal Economic Society, 1973), p. xxv. Leibniz's *Specimen Demonstrationum Politicarum pro Eligendo Rege*

Polonorum novo scribendi genere ad claram certitudinem exactum is published in the original Latin in *Sämtliche Schriften und Briefe*, series 4, vol. 1, ed. Prussian Academy of Sciences (Darmstadt: Otto Reichl Verlag, 1931,), pp. 3–98; for editorial comment, see this volume, pp. xvii–xx, and vol. 2, ed. German Academy of Sciences at Berlin (Berlin: Academie-Verlag, 1963), pp. 627–635. This text is not included in Patrick Riley (ed.): *Political Writings of Leibniz* (Cambridge/London/New York: Cambridge University Press, 1972), nor is there a reference to it in the editor's introduction and notes.

An exception to the general rule is Eric Aiton: *Leibniz: A Biography* (Bristol: Adam Hilger, 1985), which has a brief discussion of the *Specimen*; more typical of those works on Leibniz that mention the *Specimen* at all is C.D. Broad: *Leibniz: An Introduction*, ed. C. Lewy (Cambridge: Cambridge University Press, 1975), p. 3: "Among his minor achievements was to produce a geometrical argument to prove that the electors to the monarchy of Poland ought to choose Philip Augustus of Neuburg as king."

I have completed a full-length study of Leibniz's *Specimen* and its significance, to be published (in 1992) in *History and Philosophy of Science.*

[37] Godfried Wilhelm Leibniz: *Die Philosophischen Schriften*, ed. C.I. Gerhardt, vol. 7 (Berlin: Weidmannsche Buchandlung, 1890), p. 200 (trans. mine). The strength of Leibniz's conviction is revealed by the number of versions which he made of this passage: cf., e.g., ibid., pp. 26, 64–65, 125; Eduard Bodemann: *Die Leibniz-Handschriften der Königlichen Öffentlichen Bibliothek zu Hannover* (Hanover: Hahn, 1895 [not 1889]; reprint, Hildesheim: Georg Olms Verlagsbuchhandlung, 1966), p. 82; Leibniz: *Opera Omnia*, ed. Ludovicus Dutens, vol. 6, part 1 (Geneva: Apud Fratres De Tournes, 1768; also reprint, Hildesheim/Zurich/New York: Georg Olms Verlag, 1989), p. 22; Leibniz: *Opuscules et fragments inédits de Leibniz: extraits des manuscrits de la Bibliothèque royale de Hanovre*, ed. Louis Couturat (Paris: Félix Alcan, Éditeur, 1903), pp. 155–156, 176. See also Louis Couturat: *La logique de Leibniz d'après des documents inédits* (Paris: Félix Alcan, Éditeur, 1901), p. 141.

[38] Hyman Alterman: *Counting People: The Census in History* (New York: Harcourt, Brace & World, 1969), esp. pp. 45–47.

[39] Henry Guerlac: "Vauban," *Dictionary of Scientific Biography*, vol. 13 (New York: Charles Scribner's Sons, 1976), p. 590, 591; on Vauban's work on "statistique et prévision," see Michel Larent: *Vauban: un encyclopédiste avant la lettre* (Paris: Berger-Levrault, 1982), pp. 132–160. Vauban's *Dixme royale*, originally published in 1707, is available in a scholarly edition, based on the original printing plus various manuscripts, E. Coornaert (ed.): *Projet d'une dixme royale, suivi de deux écrits financiers* (Paris: Librairie Félix Alcan, 1933).

[40] Francisque Bouiller (ed.): *Eloges de Fontenelle* (Paris: Garnier Frères, 1883), p. 28.

[41] See Alterman (n. 38, supra); also Helen M. Walker: *Studies in the History of the Statistical Method: With Special Reference to Certain Educational Problems* (Baltimore: Williams & Wilkins, 1929; reprint, New York: Arno Press, 1975), p. 32. This valuable work should be supplemented by Stephen M. Stigler: *The History of Statistics: The Measurement of Uncertainty before 1900* (Cambridge/London: The Belknap Press of Harvard University Press, 1986); and John A. Koren: *The History of Statistics: Their Development and Progress in Many Countries* (New York: The Macmillan Company, 1918; reprint, New York: Burt Franklin, 1970).

For an understanding of the numeracy of the age of Graunt and Petty, see especially

John Brewer: *The Sinews of Power: War, Money and the English State, 1688–1783* (New York: Alfred A. Knopf, 1989; paperback reprint, Cambridge: Harvard University Press, 1990), ch. 8, "The Politics of Information: Public Knowledge and Private Interest." The Knopf edition is used here; there are also two British editions: London: Century Hutchinson, 1988; London/Boston: Unwin Hyman, 1989. See, further, Keith Thomas: "Numeracy in Early Modern England," *Transactions of the Royal Historical Society*, 1987, **37**: 103–132.

[42] A thorough account of the Bills of Mortality may be found in Charles Henry Hull (ed.): *The Economic Writings of Sir William Petty, together with Observations upon the Bills of Mortality more probably by Captain John Graunt*, 2 vols. continuously paginated (Cambridge: Cambridge University Press, 1899; reprint, Fairfield [N.J.]: Augustus M. Kelley, 1986), pp. lxxx–xci.

[43] The fifth edition (London, 1676) of Graunt's *Observations* is reprinted in Hull's edition of Petty, pp. 314–435. The first edition (London, 1662) has been reprinted in facsimile (New York: Arno Press, 1975). Hull (pp. xxxiv–xxxviii) has assembled all the information about Graunt's life and on the authorship of the *Observations upon the Bills of Mortality*. Hull concludes that Graunt was "in every proper sense the author of the *Observations*," but he assembles evidence that Petty had an important role in the actual composition of the book, in addition to providing Graunt with medical and other information.

A later analysis of this question by Major Greenwood: *Medical Statistics from Graunt to Farr* (Cambridge: Cambridge University Press, 1948; reprint, New York: Arno Press, 1977), contains (pp. 36–39) an updated discussion of whether Graunt wrote "the book published over his name." Greenwood reviews the history of the question and lists in chronological order some studies relating to this controversy from 1925 to 1937. He concludes that Graunt was indeed the author but that a life-table in Graunt's *Observations* may have originated with Petty, the argument being that it is "far too conjectural to have been the work of so cautious a reasoner as Graunt."

[44] The importance of climate and air for health was a major feature of medical thought from the time of Hippocrates, whose treatise on "Airs, Waters, Places" continued to exert a significant influence up to the end of the eighteenth century.

[45] Hull (n. 42 supra) discusses "Graunt and the Science of Statistics" on pp. lxxxv–lxxix. Stigler (n. 41 supra), p. 4, remarks that Graunt's *Observations* "contained many wise inferences based on his data, but its primary contemporary influence was more in its demonstration of the value of data gathering than on the development of modes of analysis."

[46] Petty's *Political Arithmetick* is reprinted in volume one of Hull's edition (n. 42 supra). An important recent study of Petty is Peter H. Buck: "People Who Counted: Political Arithmetic in the Eighteenth Century," *Isis*, 1982, **73**: 28–45. Petty's work is also discussed in histories of probability and statistics, e.g., Walker (n. 41 supra).

Petty is esteemed today for his writings on economics as much as for his work on demography and political arithmetic. In economics, Petty is noted for an early statement of the doctrine of "division of labor." See William Letwin: *The Origins of Scientific Economics: English Economic Thought, 1660–1776* (London: Methuen & Co., 1963; reprint, Westport: Greenwood Press, 1963), ch. 6.

An extremely valuable resource for Petty studies, containing a wealth of information drawn from otherwise unused manuscript sources, is Lindsay Gerard Sharp: *Sir William Petty and Some Aspects of Seventeenth Century Natural Philosophy* (Unpublished D. Phil. Thesis, Faculty of History, Oxford University, deposited in the Bodleian Library 2.2.77). Scholars in many fields will regret that this important study was never published.

A useful reference source is Sir Geoffrey Keynes: *A Bibliography of Sir William Petty, F.R.S., and of Observations on the Bills of Mortality by John Graunt, F.R.S.* (Oxford: Clarendon Press, 1971).

47 Quoted from Lord Edmund Fitzmaurice: *Life of Sir William Petty, chiefly from Private Documents hitherto unpublished* (London: John Murray, 1895), p. 158. Petty used the term "political arithmetick" even earlier, in print, in his *Discourse of Duplicate Proportion* (London, 1674), and, at an earlier date, in a letter to Lord Anglesey, 17 December 1672. See Hull (n. 42 supra), p. 240n.

48 *Political Arithmetick*, preface, in Hull (n. 42 supra), p. 244.

49 In a letter to Edward Southwell, 3 November 1687, Petty described at length what algebra is. After giving an explanation of the principles and a number of examples, he concluded with a brief history, tracing the origins to Archimedes and Diophantus but noting that "Vieta, DesCartes, Roberval, Harriot, Pell, Outread, van Schoten and Dr. Wallis have done much in this last age." He then noted that algebra "came out of Arabia by the Moores into Spaine and from thence hither, and W[illiam] P[etty] hath applyed it to other then purely mathematicall matters, viz: to policy by the name of *Politicall Arithmitick*, by reducing many termes of matter to termes of number, weight, and measure, in order to be handled Mathematically." These two remarks of Petty are excerpted from the Petty-Southwell Correspondence in *The Petty Papers: Some Unpublished Writings of Sir William Petty*, ed. by Marquis of Lansdowne, 2 vols. (London: Constable & Company; Boston/New York: Houghton Mifflin Company, 1927), vol. 2, pp. 10–15; cf. pp. 3–4.

50 Hull (n. 42 supra), p. 460.

51 Ibid.

52 Ibid., p. lxvii, n. 6.

53 Ibid., p. lxviii.

54 Ibid.

55 Brewer (n. 41 supra), p. 223.

56 Hull (n. 42 supra), pp. 451–478, esp. p. 473.

57 Ibid., p. 501.

58 Ibid., pp. 521–544.

59 Of all the thinkers presented in this chapter, Hobbes is the one most familiar to students of social or political thought. Furthermore, it is generally known that Hobbes based his system on the new physics of motion, but less attention has been paid to his use of Harveyan physiology. Hence my presentation of Hobbes's use of the natural sciences stresses the biomedical basis of his political thought rather than his use of mathematics and the physical sciences.

There are many good presentations of the thought of Hobbes, among them Leo Strauss: *The Political Philosophy of Hobbes: Its Birth and Its Genesis*, trans. from the German manuscript by Elsa M. Sinclair (Oxford: The Clarendon Press, 1936; Chicago: University of Chicago Press, 1966); Arnold A. Rogow: *Thomas Hobbes: A Radical in the Service of Reaction* (London/New York: W.W. Norton & Company, 1986). There is much to be learned from two volumes by C.B. Macpherson: *The Political Theory of Possessive Individualism, Hobbes to Locke* (Oxford: Clarendon Press, 1962), and *Democratic Theory: Essays in Retrieval* (Oxford: Clarendon Press, 1973).

Especially important in the present context is an essay by J.W.N. Watkins: "Philosophy and Politics in Hobbes," *Philosophical Quarterly*, 1955, **5**: 125–146; expanded into the book *Hobbes's System of Ideas: A Study in the Political Significance of Philosophical Theories* (London: Hutchinson & Co., 1965; 2d ed., 1973). Also Thomas A. Spragens: *The Politics of Motion: The World of Thomas Hobbes* (London: Croon Helm, 1973); and M.M. Goldsmith: *Hobbes's Science of Politics* (London/New York: Columbia University Press, 1966).

Also David Johnston: *The Rhetoric of Leviathan: Thomas Hobbes and the Politics of Cultural Transformations* (Princeton: Princeton University Press, 1986); Tom Sorell: *Hobbes* (London/New York: Routledge & Kegan Paul, 1986 – The Arguments of the Philosophers); Richard Tuck: *Hobbes* (Oxford/New York: Oxford University Press, 1989 – Past Masters); and Frithiof Brandt: *Thomas Hobbes' Mechanical Conception of Nature* (Copenhagen: Levin & Munksgaard, 1928).

[60] Hobbes's *Leviathan*, his major work, is available in many editions and reprints, among them the Pelican Classics edition, ed. C.B. Macpherson (Harmondsworth: Penguin Books, 1968). The most recent edition, ed. Richard Tuck (Cambridge/New York: Cambridge University Press, 1991) has indexes of subjects and of names and places and a concordance with earlier editions.

The writings of Hobbes have been collected in two sets – Sir William Molesworth (ed.): *The English Works of Thomas Hobbes*, 11 vols. (London: John Bohn, 1839–1845; reprint, Aalen [Germany]: Scientia, 1962); Sir William Molesworth (ed.): *Thomae Hobbes Malmesburiensis Opera Philosophica Quae Latine Scripsit Omnia*, 5 vols. (London: John Bohn, 1839–1845; reprint, Aalen [Germany]: Scientia, 1961). There are also articles on Hobbes in the *Encyclopaedia of the Social Sciences*, vol. 4 (New York: The Macmillan Company, 1937), and the *International Encyclopedia of the Social Sciences*, vol. 6 (U.S.A.: The Macmillan Company & The Free Press, 1968).

[61] *English Works* (n. 60 supra), vol. 7, pp. 470–471.

[62] On Hobbes's optics, se Alan E. Shapiro: "Kinematic Optics: A Study of Wave Theory of Light in the Seventeenth Century," *Archive for History of Exact Sciences*, 1973, **11**: 134–266.

[63] "Epistle Dedicatory," *De Corpore*, in *English Works* (n. 60 supra), vol. 1, p. viii.

[64] Ibid. It should be noted that in these two referrences to his own place in history, Hobbes refers specifically to his *De Cive*, not to *Leviathan*.

[65] On the Cartesian notion of inertia, see A. Koyré: *Galilean Studies* (n. 5 supra), part 3, "Descartes and the Law of Inertia." See also the works by R. Dugas and W. Shea cited in n. 11 supra.

[66] *English Works* (n. 60 supra), vol. 6, p. 3.

[67] *Leviathan*; ch. 4, Tuck ed. (n. 60 supra), p. 28. Hobbes learned geometry only late in life and was never a real master of the subject.

[68] On the style of the writers on mechanics of the late Middle Ages, see Marshall Clagett: *The Science of Mechanics in the Middle Ages* (Madison: University of Wisconsin Press, 1959); also John E. Murdoch and Edith D. Sylla: "The Science of Motion," pp. 206–264 of David C. Lindberg (ed.): *Science in the Middle Ages* (Chicago/London: University of Chicago Press, 1978).

[69] The mathematician John Wallis kept up a continual exposure of Hobbes's attempts to square the circle. Although it had not then been proved that the squaring of the circle was impossible, no mathematician "worthy of his salt" in the seventeenth century would believe such a feat to be possible. On Wallis's attack on Hobbes for his attempts to square the circle, see J.F. Scott: *The Mathematical Work of John Wallis* (London: Taylor and Francis, 1938), pp. 166–172.

[70] Goldsmith (n. 59 supra), p. 228.

[71] *English Works* (n. 60 supra), vol. 1, pp. 406–407; see *Leviathan*, Tuck ed. (n. 60 supra), p. 3; Spragens (n. 59 supra), p. 69.

[72] *Leviathan*, ch. 5; Tuck ed. (n. 60 supra), p. 36.

[73] Ibid., p. 31.

[74] Ibid., p. 34.

[75] Ibid., p. 35.

[76] *English Works* (n. 60 supra), vol. 3, p. 35.

[77] Ibid., vol. 2, p. iv.

[78] Ibid.

[79] See Brandt, Goldsmith, Sorell, Tuck, Watkins (see n. 59 supra).

[80] Spragens (n. 59 supra), *De Corpe*, I. vi, 7, English Works, vol. 1, p. 74.

[81] *Six Lessons to the Professors of Mathematics* (Ep. Ded.), *English Works* (n. 60 supra), vol. 7, p. 184.

[82] *Leviathan*, ch. 29; Tuck ed. (n. 60 supra), pp. 228–230.

[83] Ibid., ch. 24; Tuck ed. (n. 60 supra), pp. 174–175. Although Hobbes does say that the blood that passes through the heart, before being pumped out again into the arteries, "is made Vitall," he does not indicate that the blood entering the heart from the parts of the body is made to pass out into the lungs and then back again into the heart before going out into the parts of the body once again. He does not make use of Harvey's observation that the alteration of the blood does not occur as it passes through the heart but is a result of the pulmonary transit or passage through the lungs in what is sometimes known as the lesser circulation or pulmonary circulation. Nor does Hobbes indicate that there is an observable physical difference between the blood entering the heart from the lungs and the blood coming into the heart from the various other parts of the body.

[84] Leonora Cohen Rosenfield: *From Beast-Machine to Man-Machine: Animal Soul in French Letters from Descartes to La Mettrie* (New York: Oxford University Press, 1941).

[85] Tom Sorell: "The Science in Hobbes's Politics," pp. 67–80 of G.A.J. Rogers & Alan Ryan (eds.): *Perspectives on Thomas Hobbes* (Oxford: Clarendon Press, 1988), esp. p. 71.

[86] C.B. Macpherson: "Harrington's 'Opportunity State,'" reprinted from *Past and Present* (no. 17, April 1960) in Webster (n. 19 supra), pp. 23–53, esp. p. 23. This essay is essentially reproduced as pp. 160–193 of Macpherson's *Possessive Individualism* (n. 59 supra).

[87] Richard H. Tawney: "Harrington's Interpretation of His Age," Proceedings of the British Academy, 1941, **27**: 199–223, esp. p. 200.

[88] Harrington's influence on American political organization is presented in H.F. Russell Smith: *Harrington and His Oceana: A Study of a 17th Century Utopia and Its Influence in America* (Cambridge: Cambridge University Press, 1914). See also Theodore Dwight: "James Harrington and His Influence upon American Political Institutions and Political Thought," *Political Science Quarterly*, 1887, **2**: 1–44.

[89] Charles Francis Adams (ed.): *The Works of John Adams, Second President of the United States: With a Life of the Author*, vol. 4 (Boston: Charles C. Little and James Brown, 1851 – reprint, New York: AMS Press, 1971), p. 428.

[90] James Harrington: *Works: The Oceana and Other Works*, ed. John Toland, with an appendix containing more of Harrington's political writings first added by Thomas Birch in the London edition of 1737 (London: printed for T. Becket, T. Cadell, and T. Evans, 1771; reprint, Aalen [Germany]: Scientia Verlag, 1980); cited here as Toland. For a brief listing of printings and editions of Toland's collection, see Blitzer (n. 93 infra), pp. 338–339, and for a fuller account see J.G.A. Pocock (ed.): *The Political Works of James Harrington* (Cambridge/London/New York: Cambridge University Press, 1977), pp. xi–xiv; this edition by Pocock is cited here as Pocock and is used for quotations from Harrington's text. Examples of the kinds of changes which Toland made in Harrington's text are given in n. 97 infra. Of the Toland editions, I have consulted, in addition to the reprint listed above, the original collection by John Toland: *The Oceana of Iames Harrington and His Other Works* (London: Printed [by J. Darby], and are to be sold by the Booksellers of London and Westminster, 1700); *The Oceana and Other Works of Iames Harrington*, 3rd ed., with Thomas Birch's appendix of political tracts by Harrington (London: Printed for A. Millar, 1747); *The Oceana and Other Works of James Harrington* (the London edition of 1771 as noted above); and *The Oceana of James Harrington, Esq., and His Other Works*, with the addition of *Plato Redivivus* (Dublin: Printed by R. Reilly for J. Smith and W. Bruce, 1737). Adams's library contained two printings of Toland's Harrington: The London edition (3rd ed.) of 1747 and the London edition of 1771; see *Catalogue of the John Adams Library in the Public Library of the City of Boston*, ed. Lindsay Swift (Boston: published by the Trustees, 1917).

[91] *Works* (see n. 90 supra), p. xv.

[92] Pocock (n. 90 supra), p. 164; also Toland (n. 90 supra), p. 37; also James Harrington: *Oceana*, ed. S.B. Liljegren (Heidelberg: Carl Winters Universitätsbuchhandlung, 1924 – Skrifter utgivna av Vetenskaps-societeten i Lund, no. 4; reprint, Westport, Conn.: Hyperion Press, 1979), p. 15; also *Works* (n. 90 supra), p. 37. This last edition is cited as Liljegren. I have also consulted James Harrington: *The Common-Wealth of Oceana* (London: printed by J. Streater for Livewell Chapman, 1656); on this and the other "first edition," see Pocock (n. 90 supra), pp. 6–14. The text of *Oceana* and *A System of Politics* from Pocock's edition of all of Harrington's political works (1977; n. 90 supra), have been reprinted, with a new introduction, as James Harrington: *The Commonwealth of Oceana and A System of Politics*, ed. J.G.A. Pocock (Cambridge: Cambridge University Press, 1992). There is also a useful edition by Charles Blitzer of *The Political Writings of James Harrington: Representative Selections* (New York: The Liberal Arts Press, 1955).

[93] Judith N. Shklar: "Harrington, James," *International Encyclopedia of the Social Sciences*, vol. 6 (New York: The Macmillan Company & The Free Press, 1968), p. 323; Russell Smith (n. 88 supra). Charles Blitzer's *An Immortal Commonwealth: The Political Thought of James Harrington* (New Haven: Yale University Press, 1960) is the best informed and most authoritative work on Harrington and is cited as Blitzer; a convenient list of Harrington's publications is given on pp. 337–339. Also worth consulting is Charles Blitzer's doctoral thesis: "The Political Thought of James Harrington (1611–1677)" (Harvard University, 1952). A useful briefer presentation is given in Michael

Downs: *James Harrington* (Boston: Twayne Publishers, 1977). An important critical survey of interpretations of Harrington is given in J.G.A. Pocock: *Politics, Language, and Time: Essays on Political Thought and History* (Chicago: The University of Chicago Press, 1989), ch. 4, "Machiavelli, Harrington, and English Political Ideologies in the Eighteenth Century."

Harrington opposed the idea that the state should be modelled on a machine or constructed on mathematical principles. His attack was obviously directed at Hobbes, who appears in *Oceana* as an almost omnipresent target under the name "Leviathan." It has recently been argued, however, that Harrington was, to a considerable degree, a follower of the Helmontian philosophy, that he "appears Helmontian in his scorn for the use of mathematics in the 'new mechanical philosophy.'" Thus when Wren attacked Harrington for having assumed a perpetual mechanics, Harrington replied that "in the politics there is nothing mechanic or like it" and that to suppose so "is but an idiotism of some mathematician." See Wm. Craig Diamond: "Natural Philosophy in Harrington's Political Thought," *Journal of the History of Philosophy*, 1978, **16**: 387–398 esp. pp. 390, 395. Diamond argues further (e.g., p. 397) that not only was the concept of a Helmontian *spiritus* important in Harrington's philosophy of nature; "Harrington incorporated a number of related conceptions of *spiritus* within his political philosophy." Exploring Harrington's philosophy of nature from a new scholarly perspective, the author of this original and important analysis does not mention Harrington's concept of political anatomy nor does he explore Harrington's use of the science of William Harvey.

[94] Pocock (n. 90 supra), p. 656; also James Harrington: *The Art of Law-Giving: In III Books; The Third Book: Containing a Model of Popular Government* (London: Printed by J. C. for Henry Fletcher, 1659), p. 4; also Toland (n. 90 supra), p. 403.

[95] Pocock (n. 90 supra), p. 656; also *The Art of Law-Giving* (n. 94 supra), p. 4; also Toland (n. 90 supra), pp. 402–403.

[96] Pocock (n. 90 supra), p. 162; also Liljegren (n. 92 supra), p. 13; also Toland (n. 90 supra), p. 36. Cf. Harrington's *Politicaster*, in Pocock, p. 723 (and see also Toland, p. 560), where Harrington insists that "in the politics," as in anatomy, what counts is "demonstration out of nature"; politics must follow "the known course of nature."

[97] Pocock (n. 90 supra), p. 287; also Liljegren (n. 92 supra), p. 149; also Toland (n. 90 supra), p. 149. In his edition Toland has changed "store" to "matter," "gusheth" to "spouts," and "life blood" to "vital blood." That Toland and not a later editor is the author of these changes is indicated by their appearing in his edition of 1700 (n. 90 supra), p. 161. Earlier in *Oceana*, Harrington compares the Council of Trade to the Vena Porta (Pocock, p. 251; also Liljegren, p. 110; also Toland, p. 118).

[98] Judith N. Shklar: "Ideology Hunting: The Case of James Harrington," *The American Political Science Review*, 1959, **53**: 689–691.

[99] René Descartes, *Discours de la méthode*, ed. Charles Adam and Paul Taunery, vol. 6 (Paris: Léopold Cerf, Imprimeur-Éditeur, 1902; reprint, Paris: Librairie Philosophique J. Vrin, 1965), p. 47. Descartes's discussion of Harvey appears in part 5 of the *Discours de la méthode*. See René Descartes: *Treatise of Man*, French text with trans. and comm. by Thomas Steele Hall (Cambridge: Harvard University Press, 1972).

[100] Walter Pagel: *William Harvey's Biological Ideas: Selected Aspects and Historical Background* (Basel/New York: S. Karger, 1967), p. 233.

[101] See I.B. Cohen: "A Note on Harvey's 'Egg' as Pandora's 'Box,'" pp. 233–249 of Mikulás Teich & Robert Young (eds.): *Changing Perspectives in the History of Science: Essays in Honour of Joseph Needham* (London: Heinemann, 1973).

[102] See F.J. Cole: *Early Theories of Sexual Generation* (Oxford: Clarendon Press, 1930).

[103] Pocock (n. 90 supra), p. 839; also Toland (n. 90 supra), p. 470.

[104] Ibid.

[105] William Harvey: *Disputations touching the Generation of Animals*, trans. Gweneth Whitteridge (Oxford/London: Blackwell Scientific Publications, 1981), pp. 96, 99; see also William Harvey: "Anatomical Exercises in the Generation of Animals," in *The Works of William Harvey*, trans. Robert Willis (London: printed for the Sydenham Society, 1847; reprint, New York/London: Johnson Reprint Corporation, 1965 – The Sources of Science, no. 13; reprint, Philadelphia: University of Pennsylvania Press, 1989 – Classics in Medicine and Biology Series), pp. 235, 238; also William Harvey: *Anatomical Exercitations concerning the Generation of Living Creatures*, trans. (London: Printed by James Young for Octavian Pulleyn, 1653), pp. 90, 94.

[106] Whitteridge trans. (n. 105 supra), pp. 96, 101; also Willis trans. (n. 105 supra), pp. 235, 241; also 1653 trans. (n. 105 supra), pp. 89, 97.

[107] Whitteridge (n. 13 supra), p. 218.

[108] Pocock (n. 90 supra), p. 839; also Toland (n. 90 supra), p. 470.

[109] Pocock (n. 90 supra), p. 840; also Toland (n. 90 supra), p. 470.

[110] *The Prerogative of Popular Government: A Politicall Discourse in Two Books* (London: Printed for Tho. Brewster, 1658 [1657]), p. 20; also *Works* (n. 90 supra), p. 232.

[111] Pocock (n. 90 supra), p. 412; also *Prerogative* (n. 110 supra), p. 21; also Toland (n. 90 supra), p. 232.

[112] Whitteridge trans. (n. 105 supra), pp. 8–10; also Willis trans. (n. 105 supra), pp. 152–153.

[113] Whitteridge trans. (n. 105 supra), pp. 12–13; also WIllis trans. (n. 105 supra), pp. 157–158.

[114] Quoted in Kenneth D. Keele: *William Harvey: The Man, the Physician, and the Scientist* (London/Edinburgh: Nelson, 1965), p. 107.

[115] On Harvey's method see especially Walter Pagel (n. 100 supra).

[116] Pagel (see n. 100 supra), pp. 24, 331 (with qualifications, e.g., on pp. 24–25, 330–331). See also Charles Singer: *The Evolution of Anatomy* (New York: Alfred A. Knopf, 1925), pp. 174–175; Keele (n. 114 supra), p. 190.

[117] Pocock (n. 90 supra), p. 310; also Toland (n. 90 supra), p. 170.

[118] Blitzer (n. 93 supra), p. 99; Pocock (n. 90 supra), p. 723; also Toland (n. 90 supra), p. 560.

[119] Ibid.

[120] Keynes 1928 (n. 13 supra), pp. 165–166, 145; also "A Second Disquisition to John Riolan, Jun., in Which Many Objections to the Circulation of the Blood Are Refuted," trans. Robert Willis (n. 105 supra), pp. 123, 109. Whitteridge trans. (n. 13 supra), p. 7; also Willis trans. (n. 13 supra), p. 7; also Keynes 1928, p. xiii; also Keynes 1978 (n. 13 supra), p. xi.

[121] Harrington was dismayed by the fact that certain "natural philosophers" (Bishop Wilkins, for example, in his *Mathematical Magick*) wrote of machines or devices that

could either not be constructed or that could never in practice work exactly as proposed in theory; see the excellent presentation in Blitzer (n. 93 supra), pp. 90–95.

[122] Pocock (n. 90 supra), pp. 198–199; also Liljegren (n. 92 supra), p. 50; also Toland (n. 90 supra), p. 65. Cf. *Politicaster* in Pocock, p. 716; also Toland, p. 553.

[123] Keynes 1928 (n. 13 supra), p. 179; also Willis trans. (n. 120 supra), p. 132. (In *De Motu Cordis* Harvey did call "the heart of creatures" the "prince of all, the sun of their microcosm" (see n. 14 supra), but that does not mean that he favored the heliocentric system of Copernicus; cf. Whitteridge trans. (n. 13 supra), p. 76; Keynes (n. 13 supra), p. 47; Franklin trans. (n. 13 supra), p. 59. In *De Generatione Animalium*, Harvey did not compare the heart to a central sun. Rather, adopting a geocentric position (which could be Ptolemaic or Tychonic, etc.), he called the blood "the sun of the microcosm" and compared it further to "the superior luminaries, the sun and the moon," which "give life to this inferior world by their continuous circular motions." See Whitteridge translation (n. 105 supra), pp. 381–382; also Willis trans. (n. 105 supra), pp. 458–459.

[124] Keynes 1928 (n. 13 supra), p. 168; also Willis trans. (n. 120 supra), p. 124.

[125] A wholly new interpretation of Harrington's disdain for physics (mechanics) and mathematics has been suggested by Williamm Craig Diamond: "Natural Philosophy in Harrington's Political Thought," *Journal of the History of Philosophy*, 1978, **16**: 387–398. Diamond provides convincing evidence that in this regard Harrington was, to a considerable degree, a follower of the Helmontian philosophy. That is (pp. 390, 395), Harrington may have been "Helmontian in his scorn for the use of mathematics in the 'new mechanical philosophy.'" Diamond argues further (e.g., p. 397) that not only was the concept of a Helmontian *spiritus* important in Harrington's philosophy of nature; "Harrington incorporated a number of related conceptions of *spiritus* within his political philosophy." Exploring Harrington's philosophy of nature from a new scholarly perspective, the author of this original and important analysis does not, however, mention Harrington's concept of political anatomy, nor does he explore Harrington's use of the science of William Harvey in either forming a philosophy of nature or a system of political thought.

[126] Whitteridge trans. (n. 13 supra), p. 7; also Willis trans. (n. 13 supra), p. 7; Franklin trans. (n. 13 supra), p. 7; Keynes 1928 (n. 13 supra), p. xiiil; Keynes 1978 (n. 13 supra), p. xi. Harrington made other references to Harvey, even – as Robert Frank noted – using "the discovery of the circulation to argue society's need for an innovator, in this case a single legislator to lay down a plan of government"; see Robert G. Frank, Jr.: "The Image of Harvey in Commonwealth and Restoration England," pp. 103–143 of Jerome J. Bylebyl (ed.): *William Harvey and His Age: The Professional and Social Context of the Discovery of the Circulation* (Baltimore/London: The Johns Hopkins University Press, 1979), esp. p. 120. In this context Frank quotes from Harrington's *The Prerogative of Popular Government*: "*Invention* is a solitary thing. All the Physicians in the world put together invented not the circulation of the bloud, nor can invent any such thing, though in their own Art; yet this was invented by One alone, and being invented is unanimously voted and embraced by the generality of Physicians." This treatise by Harrington is included in Pocock (n. 90 supra).

[127] Petty and Graunt are exceptions in that almost all of their writings are devoted to topics in science or mathematics in relation to general polity.

NOEL M. SWERDLOW

5. BLACKSTONE'S "NEWTONIAN" DISSENT

The contribution of the sciences to the law is today so well established that one may wonder how courts ever managed to reach decisions without the special wisdom of physicists and chemists, biologists and ecologists, psychologists, sociologists, engineers of the most arcane expertise, an occasional mathematician, and, of course, physicians beyond counting. For plaintiff and defendant, for state and accused, through deposition and testimony, for fee and for free, a legion of learned professors and practitioners expound the abstruse technicalities of scientific truth in the most simple or the most impenetrable language depending upon whether the purpose of their assistance is to clarify or confuse, to enlighten or astound. Let an airplane fall down or a tooth-filling fall out, let an electronic computer or an adjustable wrench be mysteriously duplicated, and the courtroom will fill with projectors and blackboards, diagrams and displays, taking on the appearance of a class in anything from remedial arithmetic to advanced fluid dynamics.

The professions of law and science have remained on generally good terms because the former has sufficiently complimented the latter through its limitless (and expensive) search for such absolute truth, and perhaps because each is sufficiently uninstructed in the other's knowledge and skills to maintain a certain distant respect. But it was not always so. Aside from the occasional physician, who appeared more as a representative of the standards of a profession than as a witness to scientific knowledge, prior to the patent cases of the last century or so, courts had little need of scientific evidence, for to search the law reports for any but the legal science, and some would say that there is little enough of that, is not a rewarding exercise. While law may indeed have considered itself a science, it had not yet taken on a "sociable disposition" in its relation to the other sciences. It is the distinction of William Blackstone (1723–80), from whom the previous phrase was borrowed, to have drawn attention to the beneficial relation to law of the academical sciences. He did so in the introduction, *On the Study of the Law*, to the Vinerian Lectures, read at Oxford in 1758 and later published in his *Commentaries on the Laws of England*, by way of demonstration that the study of

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 205–234.

law, meaning here the common law, properly belongs in the universities.[1]

The advantages that might result to the science of law itself, when a little more attended to in these seats of knowledge, perhaps would be very considerable. The leisure and abilities of the learned in these retirements might either suggest expedients, or execute those dictated by wiser heads, for improving it's method, retrenching it's superfluities, and reconciling the little contrarieties, which the practice of many centuries will necessarily create in any human system: a task, which those, who are deeply employed in business and the more active scenes of the profession, can hardly condescend to engage in. . . . For the sciences are of a sociable disposition, and flourish best in the neighbourhood of each other: nor is there any branch of learning, but may be helped and improved by assistances drawn from other arts.

Blackstone goes on to point out the importance to the education of a lawyer of the purest classical writers, particularly, historians and orators, the clear simple rules of logic, the use of mathematical demonstrations, the several branches of experimental philosophy, the maxims of the law of nature – the best and most authentic foundation of human laws – and lastly the laws of imperial Rome.[2] And in a later defence of the language of the law – law French, law Latin, terms of art in whatever language – he remarks that "my academical readers will excuse me for suggesting, that the terms of the law are not more numerous, more uncouth, or more difficult to be explained by the teacher, than those of logic, physics, and the whole circle of Aristotle's philosophy, nay even of the politer arts of architecture and it's kindred studies, or the science of rhetoric itself."[3]

Beyond the preparatory value of the sciences in legal education, Blackstone himself attempted a curious integration of law and science in the *Commentaries*, something not unusual at his time, that has led to possibly the strongest criticism of his work, unkind remarks to the effect that for him the law of gravitation and the Rule in Shelley's Case are all one and the same. We shall consider his general observations later in this essay, but our principal subject, and what appears a far more remarkable introduction of scientific learning into the law, is a dissenting opinion by Blackstone when a Justice of the Court of Common Pleas that, without considering its scientific merit, has been subject to perhaps as many strictures as his attempt at scientific and philosophical learning in the *Commentaries*. The dissent was delivered in the famous squib-throwing case, long familiar to law students, at least in an abridged form, from case books on torts and civil procedure, that has ever since produced both discussion and, not without reason, dissatisfaction.

THE CELEBRATED CASE OF SCOTT V. SHEPHERD[4]

The facts of the case, according to the declaration of the plaintiff, are as follows:[5]

A week before Guy Fawkes Day, in the evening of 28 October 1770, the day of the fair at Milbourne Port in Somerset, one Shepherd, an infant (i.e. under twenty-one years of age), threw from the street into the crowded market-house, a covered building open at the sides, a lighted serpent, being a large squib or firecracker filled with gunpowder and other combustible materials. This act, as Justice Nares later remarked, "was of a mischievous nature, and bespeaks a bad intention." The squib fell upon the standing of one William Yates, who sold gingerbread, cakes, pies, and other pastries. Instantly it was picked up by one James Willis who, to prevent injury to himself and Yates's wares, threw the squib across the market-house, whence it fell upon the standing of one James Ryall, who sold the same sort of wares. Ryall likewise, to save himself and his goods from injury, took up the squib and threw it to another part of the market-house, whence it struck the face of one Scott, also an infant, and then bursting, burned him severely and put out one of his eyes.

So Scott, by his next friend, brought an action of trespass and assault against Shepherd, by his guardian, asking £500 damages, which action, in a little under two years, was tried before Justice Nares at the Summer Assizes of 1772 at Bridgewater. The defendant pleaded the general issue, not guilty – which means that he denied the entire declaration, that he did no act that gave injury to the plaintiff[6] – and upon the evidence the jury found a verdict for the plaintiff with £100 damages, subject to the opinion of the court whether these facts could maintain an action of trespass. The question was argued before the Court of Common Pleas in Hilary Term of 1773, Serjeant John Glynn for the plaintiff and Serjeant John Burland for the defendant, and the issue to be decided, whether the proper action was one of *trespass vi et armis* or *trespass on the case*, is set forth clearly enough in the summary of the arguments of counsel reported by Wilson. It was objected at trial that trespass *vi et armis* was the wrong action since the injury received by the plaintiff was not the *immediate* act of Shepherd, but was *consequential*, and probably would not have happened had the squib not been thrown by Willis and then by Ryall, for which the proper remedy was by an action on the case. Could the injury be considered the immediate act of the defendant or, if not, could he nevertheless be held responsible in an action of trespass? Otherwise the plaintiff's suit would fail for want of the proper action.

The action of trespass (*transgressio*, step beyond) developed in the thirteenth century as the remedy for injuries to persons or property accompanied, if only in theory, by force or violence.[7] Trespass fell into broad categories set out in the writ: for entry to land trespass *quare clausum fregit*, for taking or detaining personal property trespass *de bonis asportatis*, for personal injury or detention trespass *vi et armis*. Because the acts were considered violent and unlawful *per se*, the writs usually contained the words *contra pacem*, indicating that they were also offenses against the King's peace for which a fine, even if nominal, was levied. The defense to these actions was for the defendant to deny that he had done the act at all, or to claim some right to do the act, as title to the land or property or striking the plaintiff in self defense. But what if the act resulting in the injury fits none of the preceding categories, is not unlawful in itself and not accompanied by force, even in theory, and cannot be called *contra pacem*? For example, a farrier in shoeing a horse makes it lame, or refuses to return a horse put in his keeping, or a horse stumbles over logs laid in the road and its rider injured, or an innkeeper sells bad provisions and a customer falls ill, or a patient is harmed by the *mala praxis* of a physician, surgeon or apothecary. For these injuries there developed a very general action called trespass on the case (*transgressio super casum*) in which the cause of complaint is specifically set out in the original writ and the words *contra pacem* are not used. Although other actions were available, case became a common remedy for injuries resulting from omission, failure in an undertaking, and negligence. The general distinction of trespass and case seems to have been twofold: trespass lay for forceful, and therefore unlawful, acts causing *immediate* injury; case for acts or omissions in themselves lawful, because not forceful, that were injurious by some *consequence* of the act or failure to act. There are two difficult, or interesting, cases: 1. If a lawful act is immediately injurious, in other words, an accident, is it a trespass? This goes to the question of the standard of liability, and earlier decisions generally upheld strict liability, every man acts at his own peril.[8] 2. If an unlawful act, an act of the nature of trespass, causes injury, not immediately, but consequentially, is the proper action trespass or case? This goes both to the procedural question of the distinction of the actions and also to the standard of liability for the consequences of an act however remote or unforeseen. Here the case law was less clear, and this was the issue in *Scott* v. *Shepherd.*

Serjeant Glynn argued that Shepherd was responsible at all events since "whoever does a *tortious* act is answerable in trespass *vi et armis* for all the consequences," as if a man turns loose amongst people an unruly ox, a lion or a tiger and mischief ensues the person injured may have trespass *vi et armis*.[9] If a man throws a stone over a wall and kills another it is manslaughter, although he neither saw nor aimed at any body. Further, the act of the defendant was unlawful by statute for, by 9 & 10 William 3 c. 7, "the throwing of any squibs in any public street, house, shop, river, highway or passage shall be adjudged a common nuisance, and every person being convicted thereof shall forfeit 20*s* and if he does not immediately pay the same shall be committed to the house of correction." Hence the act of the defendant was unlawful, and mischief having ensued, trespass *vi et armis* well lies against him.

In answer for the defendant Serjeant Burland went to the matter of immediacy, a requirement in an action of assault and battery, arguing that the two intervening parties made the injury consequential, thereby taking it out of trespass.[10] If the squib had not been touched by the second man after it had been thrown by the defendant, it might have expired and done no harm. "No act hath been done by the defendant to the plaintiff from whence the injury happened." He agreed that in the case put of turning loose a wild beast trespass would lie, for this is the very act of the person and is as much an assault and battery as shooting a bullet. Likewise for striking a horse and driving it over another man "for injury and hurt is the necessary consequence of the act." But here the squib could not have harmed the plaintiff had not a second and a third man thrown it, "and I humbly insist that an action of assault and battery would have laid against the man who last threw it at the plaintiff; as if one throws a stone, but hurts no body, and another takes it up and throws it again and thereby hurts a third person, the action must lie against the person who threw it secondly, and not against the first person who did no harm." He pointed out that Ryall could not have defended himself by pleading that he threw the squib *causaliter, et per infortuniam et contra voluntatem suam* (accidentally, and by misfortune and against his will), for in the case of *Weaver* v. *Ward* (1616),[11] an injury caused by an accidental discharge of a musket among trained soldiers while skirmishing, this was resolved no good plea. "So I say as Ryall is guilty of the immediate assault, the defendant is not; he is only guilty of a nuisance, by first throwing the squib."

It is easy to see that the analogies chosen by the attorneys, although acceptable enough for the sake of argument, are open to objection. In the example of the wild animal, whoever sets it loose is the last person to control the beast; not so the defendant's throwing of the squib, particularly if the throwing by Willis and Ryall was not entirely a matter of self defense. On the other side, the stone that hurts no one poses no danger to the second thrower, unlike the squib, and neither does the musket case address the question of self defense which clearly requires a more careful consideration before exonerating Shepherd to the detriment of Ryall. But the central issue, that on the one hand, he who does a tortious act is liable for its consequences while, on the other, trespass lies only for an immediate injury, is drawn clearly.

The court took some time to consider the question, handing down its decision in Easter Term when judgment was given for the plaintiff by three justices against one, Blackstone. All four opinions show the difficulty of arriving at a clear distinction, or better, a clear justification of a distinction, between trespass and case when an initially wrongful act, that is, an act in the nature of a trespass, only becomes injurious through the intervention of mediating causes, in this case rational agents. The question is whether the initial wrong or the intervening causes should govern the proper form of action. Justices Nares and Gould rested their opinion on the former, Blackstone and Chief Justice De Grey on the latter, although De Grey ultimately also lands upon the unlawfulness of the original act. That at least one of the justices who found for the plaintiff was not going to let fine distinctions set aside a just verdict seems certain.

The earlier case relied upon most particularly, and that for both interpretations no less, is *Reynolds* v. *Clarke* (1725),[12] a case notable both for the contradictions in its reporting and for the confusion of the court in reaching a decision. Here the defendant entered his neighbor's yard, for which he had an easement to use the well and pump, and erected on the side of his own house a rain spout through which rainwater flowed into the yard, flooding and rotting out the walls of the neighbor's stable and brewhouse. The neighbor as plaintiff brought trespass *vi et armis*, his counsel arguing that erecting the spout constituted an unlawful enlargement of the easement and thus a trespass *ab initio* from which the injury followed. Counsel for the defendant claimed that the initial entry and act was lawful and in itself harmless, so trespass cannot lie, but recovery for any injury must be brought as an action on the case. Initially the justices were divided two and two as to whether the act of the defendant were lawful or itself a trespass, but after further consideration all four held for the defendant, that trespass would not lie and the proper action was on the case. However, their distinctions of trespass and case differed, for according to Chief Justice Raymond, joined by Justice Powys:

> We must keep up the boundaries of actions, otherwise we shall introduce the utmost confusion: if the act in the first instance be unlawful, trespass will lie; but if the act is prima facie lawful (as it was in this case) and the prejudice to another is not immediate, but consequential, it must be an action upon the case; and this is the distinction.[13]

Justice Fortescue, however, held that

> . . . trespass will not lie for procuring another to beat me; if a man throws a log into the highway, and in that act it hits me; I may maintain trespass, because it is an immediate wrong; but if as it lies there I tumble over it, and receive injury, I must bring an action upon the case; because it is only prejudicial in consequence, for which originally I could have no action at all.[14]

And Justice Reynolds agreed, for

> . . . the distinction is certainly right; this is only injurious in its consequence, for it is not pretended that the bare fixing a spout was a cause of action, without the falling of any water; the right of action did not accrue till the water was actually descended, and therefore this should have been an action upon the case.

Although not the deciding issue in *Reynolds* and *Clarke*, Lord Raymond clearly stated the principle that "if the act in the first instance be unlawful, trespass will lie", and in *Scott* v. *Shepherd*, Justice Nares, after acknowledging the question of immediate or consequential injury, takes the distinction of trespass and case to be between lawful and unlawful acts, citing Lord Raymond as authority.[15] Shepherd's act, he said, made it "highly probable that some personal damage would immediately happen to somebody," and this action he believed to be illegal at common law while the statute 9 & 10 W. 3 c. 7 "puts it out of doubt that the act was unlawful." To the argument that the injury was the immediate act of Ryall, not the defendant, he answers that "the act of throwing the squib into the market-house was of a mischievous nature, and bespeaks a bad intention, and whether the plaintiff's eye was put out *mediately* or *immediately* thereby, the defendant who first threw the squib is answerable in this action." He enlarges upon this point by an extension of Serjeant Glynn's example of an enraged beast.

I answer that the defendant was the first actor, and the cause of the cause of the putting out the eye of the plaintiff, the act was not complete until the explosion; if a man turns out a mad bull, ox or any other wild or mischievous beast towards *A* who turns the brute towards *B* who turns it again towards *C* whom it hurts, he who was the first actor and turned out the beast is answerable in trespass *vi et armis* for the injury done to *C*.

Finally, he cites the Court in *Slater* v. *Baker and Stapleton*,[16] where it was objected that an action brought under case should have been trespass, that "we will not look with eagle's eyes to see whether the evidence applies exactly or not to the case, when we can see the plaintiff has obtained a verdict for such damages as he deserves, but will establish such verdict if possible."

Blackstone's opinion was read next, but we shall defer it for the moment. Justice Gould agreed with Justice Nares, that "wherever a man does an unlawful act, he is answerable for all the consequences; and trespass will lie against him, if the consequence be in nature of trespass."[17] Further, he held that the defendant may be considered "as if he himself had personally thrown the squib in the plaintiff's face. The terror impressed upon Willis and Ryall excited self defence, and deprived them of the power of recollection. What they did was therefore the inevitable consequence of the defendant's unlawful act." This of course exonerates Willis and Ryall since inevitability, or "inevitable necessity", is a good plea in an action of trespass. He also points out that if, as he thought, neither Willis nor Ryall are liable to an action, and this action will not lie against Shepherd who did the first act, which was unlawful, then the plaintiff, who has been greatly injured, will be without remedy.[18]

Lord Chief Justice De Grey's opinion is the most complex (or confused) for he begins by stating one distinction between the actions, but ends up basing his decision on another. He admits, however, that the issue is worthy of the most careful examination.[19]

The distinction between the actions of trespass on the case and trespass *vi et armis* should be most carefully and precisely observed, otherwise we shall introduce much confusion and uncertainty; this is *that kind* of injury where the distinction is very nice. It strikes me thus; trespass *vi et armis* lies against the person from whom an injury is received by *force*. So the question is, whether this personal injury was received by the plaintiff by *force* from the defendant? Or whether the injury was received from, or resulting from a new *force* of another?

Now this is a different criterion. By *force* is meant the *vis* in *vi et armis* (with force and arms) in the declaration for trespass to persons, but we shall see that De Grey also calls upon a mechanical analogy. He agrees with Blackstone (*infra*) that the proper distinction is between immediate and consequential injury, not whether the original act is lawful or unlawful, for a lawful act may be trespass, as in entering his neighbor's ground to take away cut thorns, and an unlawful act case, as in laying a log in the highway, a nuisance, whereby another person is hurt. But after admitting this, he turns, not merely to the question of lawfulness, but directly to criminal law, quoting Foster's *Crown Law* and invoking transferred intent.

'If an action unlawful in itself be done deliberately, with intention of mischief or great bodily harm to particulars, or of mischief indiscriminately, fall where it may, and death ensure against or beside the original intention of the party, it will be murder.' *Foster* 261.[20] So where a blow, intended against *A* with a malicious murderous intention, lights on *B* and kills him, it is murder; although the blow was not intended at *B*.

So De Grey likewise comes round to the unlawfulness of the original act, but he still must explain how to apply transferred intent through the mediation of third parties. He does it this way: The fault, he says, *egreditur e persona* (arises from the person) who first threw the squib, and the "removal" of the squib by Willis and Ryall "for fear of danger to themselves seems to me to be a continuation of the first act of the defendant until the explosion of the squib; no man contracts guilt in defending himself."

. . . all the injury was done by the first act of the defendant; here I lay the stress and here I differ with my brother Blackstone; for I conceive all the facts of throwing the squib must be considered as one single act, namely the act of the defendant; the same as if it had been a cracker made of gunpowder which had bounded and rebounded again and again before it struck out the plaintiff's eye.

Note that De Grey has modified Nares's analogy of the deflected wild beast into a sort of physical or mechanical analogy in which the squib bounces from place to place until it hits the plaintiff. But the analogy is strained when applied to mediating persons, and whatever sort of physics the Chief Justice has in mind can hardly be called strict.

Not so Justice Blackstone in his dissent.[21] He takes the distinction between trespass and case to be between immediate and consequential injury, and he really means it; but rather than saying that the two intervening parties make the injury consequential and leaving it at that, he sets out what he appears to consider a rigorous physical argument. He allows that Willis and Ryall had a right to protect themselves by removing the squib "in such a manner as not to endamage others."

But Shepherd, I think, is not answerable in an action of trespass and assault for mischief done by the squib in the new motion impressed upon it, and the new direction given it, by either Willis or Ryal; who were both free agents, and acted upon their own judgment. This differs it from the cases put of turning loose a wild beast or a madman. They are only instruments in the hand of the first agent. Nor is it like diverting the course of an enraged ox, or of a stone thrown, or an arrow glancing against a tree; because there the original motion, the *vis impressa*, is continued, though diverted. Here the instrument of mischief was at rest, till a new *impetus* and a new direction are given it, not once only, but by two successive rational agents.[22]

This is the crux of Blackstone's argument. It is remarkable that both in language and, with one bit of imprecision, in substance it is based squarely upon Newton's Fourth Definition and first two Laws of Motion from Book I of the *Principia*. The Fourth Definition is:[23]

An impressed force (*vis impressa*) is an action exerted upon a body for the purpose of changing its state either of resting or of moving uniformly in a straight line.

Obviously, as Blackstone remarks, after lighting on Yates's stall the squib was twice subject to additional impressed forces, each changing its state from being at rest. However, his statement that in the case of an ox or stone or glancing arrow "the *vis impressa*, is continued, though diverted" is rather careless (at least as far as the inanimate objects go; the ox, of course, may gore where he will), for Newton adds to the definition the explanation, "This force consists in the action alone, and does not remain in the body after the action." Newton's intention here is to distinguish the action of an impressed force from an old theory of projectile motion according to which a force, called *impetus*, remains in a projectile after it is thrown and continues its motion until the projectile's own *gravitas* (heaviness) and the resistance of the air cause it to fall to earth. That Blackstone appears to apply the old impetus theory is surely unintentional. For in Newtonian mechanics (Definition III) the motion or rest of a body is continued by its own *vis insita* (innate force) or *vis inertiae* (force of inertia or inactivity), a presumed property of matter, the effect of which is described by the First Law of Motion:[24]

Every body continues in its state of resting or of moving uniformly in a straight line except in so far as it is compelled to change its state by impressed forces (*viribus impressis*).

Blackstone seems to have this in mind when he says that "the instrument of mischief was at rest, till a new *impetus* (which he appears to use synonymously with *vis impressa*), and a new direction are given it." Further, the specification of a "new direction" as well as the earlier reference to "the new motion impressed upon it, and the new direction given it" are applications of the Second Law of Motion:[25]

The change of motion is proportional to the motive force impressed (*vi motrici impressae*), and takes place in the direction of the straight line in which the force is impressed.

Thus in summary, Blackstone's "Newtonian" argument, for what it is worth, is that since the initial force and motion impressed upon the squib by Shepherd was expended when it came to rest on Yates's stall, and since it required two additional impressed forces, by two successive rational agents no less, to give it the new motion and new direction through which it at last struck the plaintiff's face, in no way could the injury be considered the immediate act of the defendant.

One may or may not consider Blackstone's judicial reasoning satisfactory; one may or may not consider Blackstone's physical reasoning satisfactory. While there is no reason to believe that he directly consulted the *Principia* itself in writing his decision, he may once have looked at the book or he may be recalling, imperfectly but sufficiently to his purpose, some popularization he had perused years earlier. His point, after all, is not profound. Perhaps he is only reflecting common knowledge of a university graduate – at Pembroke College he read classics and wrote poetry – but there can be no doubt that he is going to some length to distinguish immediate and consequential injury, trespass and case, on the basis of what he takes to be rigorous physical reasoning. We must remember that Blackstone is not here addressing the entire question of liability – indeed, he later suggests that case would lie against Shepherd and trespass against Ryall – but the purely factual question of whether the injury was the *immediate* act of the defendant. In such an examination, the reliance upon obvious and true physical principles is not out of place, and in fact is a more rigorous and correct way of deciding the case than by analogies of diverting enraged oxen or the glancing of arrows off trees. To make his point clearer, to emphasize the remoteness of the act of the defendant from the injury of the plaintiff, Blackstone next turns to analogies along the lines of Serjeant Burland's argument, but these analogies are strictly in keeping with and illustrate his physical principles.[26]

But it is said that the act is not complete, nor the squib at rest, till after it is spent or exploded. It certainly has a power of doing fresh mischief, and so has a stone that has been thrown against my windows, and now lies still. Yet if any person gives that stone a new motion, and does farther mischief with it, trespass will not lie for that against the original thrower. No doubt but Yates may maintain trespass against Shepherd. And, according to the doctrine contended for, so may Ryal and Scott. Three actions for one single act! nay, it may be extended *in infinitum*. If a man tosses a football into the street, and, after being kicked about by one hundred people, it at last breaks a tradesman's window, shall he have *trespass* against the man who first produced it? Surely only against the man who gave it that mischievous direction.

Leaving aside for the moment consideration of the foundation of Blackstone's physical argument, is poor Scott to be without a remedy? Here too Blackstone differs with his brothers on the bench. While refraining to give a specific opinion, he suggests that case would lie against Shepherd for consequential damages, but more so, and I believe this to be the true motivation for his decision, he holds Ryall to strict liability for his act and, equally significantly, questions the intention of Willis and Ryall in throwing the squib.[27]

But I think, in strictness of law, trespass would lie against Ryal, the immediate actor in his unhappy business. Both he and Willis have exceeded the bounds of self-defence, and not used sufficient circumspection in removing the danger from themselves. The throwing it *across* the market-house, instead of brushing it down, or throwing [it] out of the open sides into the street, (if it was not meant to continue the sport, as it is called), was at least an unnecessary and incautious act. Not even menaces from others are sufficient to justify a trespass against a third person; much less a fear of danger to either his goods or his person; – nothing but inevitable necessity.

The point is well taken. If Willis and Ryall were continuing the "sport" rather than acting by "inevitable necessity", then either could have brushed the squib to the ground or thrown it into the street, and the liability does lie with Ryall, who threw it "to another part of the said market-house" where it hit Scott in the face. By this analysis, Shepherd, for all his mischievous intention, is the more remote and Ryall the immediate trespasser. My guess is that the other justices were equally aware of this conclusion, but did not wish to "look with eagle eyes to see whether the evidence applies exactly or not to the case, when we can see the plaintiff has obtained a verdict for such damages as he deserves." Another practical consideration, beginning with the original trial, may have been that the infant Shepherd was a deeper pocket than Ryall the pastry vendor, who had probably not seen £100 in his life.

SOME LAW, SOME SCIENCE, AND SOME REASON IN THE COMMENTARIES

Returning to our original subject, why should Blackstone bother to invoke physics when a simple statement of the facts, perhaps along with some analogies, would doubtless be adequate to justify his opinion that the principal liability belongs to Ryall? Pedantry? Ostentation? Possibly, and Blackstone has been accused of both, but in all fairness I think he should be granted a more serious motive for his display of learning, and in search of this we must look to the *Commentaries*, in the introduction to which he devoted some pages to considering the nature of law in general and its relation to the common law of England. No part of the *Commentaries* has been so frequently or so sharply criticized, probably because it is next to impossible to say anything convincing on so large and diverse a principle as law, but also, it must be admitted, because Blackstone's remarks on jurisprudence, resting upon uncertain assumptions and commonplaces, are philosophically not very profound. However, it is not our interest to defend or take issue with Blackstone over his explanation of the signification of law – although we find him on the whole more sensible than his philosophically more astute critics – but only to discover its relation to his dissent in *Scott* v. *Shepherd*. It is a lengthy chain of reasoning that begins very simply. Blackstone holds that *law*, whether in the physical, moral, or political world, is "a rule of action".[28]

> Law, in it's most general and comprehensive sense, signifies a rule of action; and is applied indiscriminately to all kinds of action; whether animate or inanimate, rational or irrational. Thus we say, the laws of motion, of gravitation, or optics, or mechanics, as well as the laws of nature and of nations. And it is that rule of action, which is prescribed by some superior, and which the inferior is bound to obey.

Note that Blackstone is not saying that the word "law" is used in a variety of senses for a variety of things, but that wherever the word is applied its meaning is exactly the same. Thus the laws of motion are laws and the laws of nations are laws, and while it is obvious that in each case the legislator is of a very different station, the laws are "rules of action" commanded by some superior for the compulsory obedience of some inferior.

Blackstone's unitary definition of law is superficially close to that of Montesquieu, who considered laws to be "necessary relations derived from the nature of things, and in this sense all beings have their laws:" the Deity, the material world, intelligences superior to man, beasts, and man. The effects we observe in the world are due, not to a blind fatality, which could not have produced intelligent beings, but to a primeval reason, and laws are the relations found between it and the various beings, and the relations of the beings among themselves. God is the creator and preserver of the universe; the laws by which he created are the laws by which he preserves. He acts according to these laws because he knows them; he knows them because he made them; he made them because they are consonant with his wisdom and power.[29] And indeed, in the course of *De l'esprit des lois*, I, 1–3, Montesquieu says much that seems similar to Blackstone. But the more one ponders the translucent Gallic prose of the President, the more one believes that he has never defined "law" at all, and for all his Gallic precision, the less one understands of what he means, if indeed he means anything at all, something with which we have much experience in our own day.

Whether right or wrong, profound or trite, Blackstone at least gives a clear and precise definition that says exactly what "law" is, and there is no difficulty in understanding it. Unfortunately, the definition is easy to criticize by pointing out that physical laws (I avoid the ambiguous term natural laws) are *descriptions* or *models* rather than *rules*, that such laws, i.e. such descriptions or models, are not obeyed or disobeyed (except metaphorically), but are either accurate or nearly accurate or inaccurate – the law may be wrong, but nature is always right. So much for philosophy of science. The laws of human conduct, on the other hand, be they moral or positive, may be obeyed or disobeyed, are either good or bad, and really are for the most part "rules of action" (that seems a good enough formulation). This has been stated so many times, and at such length, by Bentham and Austin and Holland and even by Blackstone's editor Christian, who dismiss the application of law to anything but human conduct as nothing but metaphor, that Blackstone's definition has become little more than an object of reproach.

Blackstone, one must confess, left himself open to such reproach by being very particular in his account of the origin of physical laws.[30]

Thus when the supreme being formed the universe, and created matter out of nothing, he impressed certain principles upon that matter, from which it can never depart, and without which it would cease to be. When he put that matter into motion, he established certain laws of motion, to which all moveable bodies must conform.

Lest there be any doubt that this is meant literally, that the supreme being may make any laws he wishes and that these must be obeyed, there is a very familiar analogy.

And, to descend from the greatest operations to the smallest, when a workman forms a clock, or other piece of mechanism, he establishes at his own pleasure certain arbitrary laws for it's direction; as that the hand shall describe a given space in a given time; to which law as long as the work conforms, so long it continues in perfection, and answers the end of it's formation.

And the same applies to the origin of the laws that govern living creatures.

If we farther advance, from mere inactive matter to vegetable and animal life, we shall find them still governed by laws; more numerous indeed, but equally fixed and invariable. The whole progress of plants, from the seed to the root, and from thence to the seed again; – the method of animal nutrition, digestion, secretion, and all other branches of vital economy; – are not left to chance, or the will of the creature itself, but are performed in a wondrous involuntary manner, and guided by unerring rules laid down by the great creator.

In all this there is nothing particularly original, although the expression is economical and elegant, for such descriptions of the Divine Legislation are as old as the notion that the universe is ruled by some sort of intelligence, and are, of course, fundamental to the arguments through design for the existence of that intelligence. Even Newton himself expresses this cosmogony in the General Scholium to Book III of the *Principia*. After describing the ordered motions of the primary planets and satellites, he says:[31]

And all these regular motions do not take their origin from mechanical causes. . . . This most elegant composition of the sun, of planets, and of comets, could not have arisen except by the counsel and dominion (*consilio et dominio*) of an intelligent and powerful being. And if the fixed stars are the centers of similar systems, all these, arranged by the like counsel, are subject to the dominion of *One*. . . . This One rules all things, not as the soul of the world, but as the Lord of absolutely all things (*universorum dominus*). And by reason of his dominion, he is wont be called Lord God Παντοκράτωρ (Almighty, omnipotent), that is Universal Ruler. For God (*deus*) is a relative term, and is taken with reference to servants (*servos*), and Deity (*deitas*) is the exercise of the ruling power (*dominatio*) of God, not over his own body, as those for whom God is the soul of the world believe, but over servants.

One may well ask what this means. Newton's principal point is that

God is not the soul of the world, that is, he is not a part of it, but is the Lord (*Dominus*) who through his own counsel (*consilium*) and dominion (*dominium*) exercises ruling power (*dominatio*) over all things as over servants (*servos*). The terms are at once Biblical and legal, from Roman civil law. *Consilium*, counsel, is advice, in civil law legal advice, and more particularly to a ruler; since God is omniscient, his advice to himself is unerring. *Dominium*, dominion, in civil law is ownership in the most complete sense with full legal power; it carries with it the absolute right of governing or sovereign authority; God's omnipotence extends his *dominium* to all things. *Dominium* is an essential attribute of God. A being however perfect, Newton later says, without *dominium* is not *Dominus Deus*, the Lord God. *Dominatio*, exercise of ruling power, is the exercise of *dominium* or sovereign authority. *Dominus*, Lord, in civil law is one who possesses *dominium*; he is the owner of a thing, the master of a slave (*servus*); God is, as it were, the owner and master of all things. Finally, *servus*, servant or slave; all things are, as it were, the servants or slaves of God by virtue of his *dominium*. We can see that the most important attribute is *dominium*, for it is God's *dominium* that gives him sovereign authority over the universe.

Blackstone need hardly have consulted the General Scholium to find God depicted as the sovereign ruler of the universe, for the theme was common enough, particularly in sermons purporting to demonstrate the Truth of Christian Revelation from the Divine Legislation of the Laws of Nature, in refutation of deists who denied the former and of atheists who denied both. A well-known example is Richard Bentley's Boyle Lectures, *The Folly and Unreasonableness of Atheism* . . . (1692–93), especially Sermons vi–viii, *A Confutation of Atheism from the Origin and Frame of the World*, that draw upon the recently revealed Newtonian philosophy.[32] Closer to home for a barrister is a collection of sermons, "vindicating Religion from the insults of Libertines, and the indiscretions of Enthusiasts", preached before the Honourable Society of Lincoln's Inn by William Warburton in 1746 and published in 1752 under the title *The Principles of Natural and Revealed Religion*, the very language of which has much in common with Blackstone's exposition of the Divine Legislation.[33]

Thus, Blackstone, with Newton, Bentley, Warburton, and surely many others besides, takes God in His essential nature to be a Lord, a Ruler, a Lawgiver who by His own will has established the laws that direct inanimate matter, animate creatures, among them man, and, as we shall see, man's moral and spiritual conduct. However, while such natural theology may provide respectable evidence for the more sublime truths of revealed religion, and may also provide a reasonable explanation for the uniformities of nature in all its manifold complexity, it is more doubtful as a foundation for moral or positive law. The reason, as mentioned before, is that "law" in a "law of nature" is a metaphor only, for the laws of motion and optics, the laws of lineal and collateral consanguinity, and the laws of contingent remainders and executory devises are not at all the same thing. Thus, when Blackstone enumerated "the laws of motion, of gravitation, of optics, or mechanics, as well as the laws of nature and of nations" as "that rule of action, which is prescribed by some superior, and which the inferior is bound to obey," he provided Jeremy Bentham with just what he required to reduce the whole notion of a Divine Lawgiver to absurdity in a draft for his *Comment on the Commentaries*.

A pleasant way enough of going to work is that the Author [Blackstone] has found out for the 'supreme being': whom unless it had been to shew his piety he might have been better employed than to trouble. Among others of this being's making are Laws of Optics. Among others that are given for Laws of Optics this is one: that the Angle of reflection *is*, say other men; (*shall be* must out Author say to make it serve him for an example) equal to the angle of incidence. We now understand how this matter was brought about. 'Hark ye', (said the Author of nature once upon a time) 'hark ye, you rays. There are some surfaces that you will meet with in your travels that when you strike upon them, will send you packing: now when in such case, this is what I would have you do: keep the same slope always in *going* that your did in coming. Mind and do what I say: if you don't, as sure as you are rays it will be the worse for you.' Upon this the rays (finding they should get into bad bread else) made their bows, shrugged up their shoulders, and went and did so.[34]

Bentham's satire, although amusing, hardly amounts to a refutation, and would doubtless have been repudiated by Blackstone and most of his contemporaries as a silly burlesque unworthy of reply. For Blackstone's philosophy of law, however naive it may appear, was at least simple, clear, and, given its modest assumption of a wise and powerful deity, entirely rational. And while we may now smile at the idea of a Divine Lawgiver to explain the reflection of light, we put nothing in its place, and in searching for a foundation of moral law, we find ourselves even more in the dark.

So much for the laws governing inanimate matter and animate but irrational creatures. Next comes human action or conduct, "the precepts by which man, the noblest of all sublunary beings, a creature endowed with both reason and free will, is commanded to make use of those faculties in the general regulation of his behaviour." Man, Blackstone holds, is entirely a dependent being necessarily subject to the laws of his creator; as an inferior, he must take the will of his superior for his rule of conduct.

This will of his maker is called the law of nature. For as God, when he created matter, and endued it with a principle of mobility, established certain rules for the perpetual direction of that motion; so when he created man, and endued him with free will to conduct himself in all parts of life, he laid down certain immutable laws of human nature, whereby that free will is in some degree regulated and restrained, and gave him also the faculty of reason to discover the purport of those laws.[35]

As a being of infinite *power*, the creator could have prescribed any laws he pleased, however unjust or severe, but as he is also a being of infinite *wisdom*, he has laid down only the eternal, immutable laws of good and evil, to which he himself conforms, and which he has enabled human reason to discover. If the discovery of these laws of nature depended upon a difficult exertion of reason, they would remain unknown to the greater part of mankind. But as the creator is also a being of infinite *goodness*, he "has so intimately connected, so inseparably interwoven the laws of eternal justice with the happiness of each individual" that the rule of obedience is reduced "to this one paternal percept, 'that man should pursue his own happiness.' This is the foundation of what we call ethics, or natural law." Thus, to know whether an act is permitted or forbidden by the law of nature, we have only to ask whether it is conductive or destructive to man's real happiness.

This law of nature, being co-eval with mankind and dictated by God himself, is of course superior in obligation to any other. It is binding over all the globe, in all countries, and at all times: no human laws are of any validity, if contrary to this; and such of them as are valid derive all their force, and all their authority, mediately or immediately, from this original.[36]

In order to apply this law of nature to the particular exigencies of each individual, it is still necessary to have recourse to reason, which, since the transgression of our first ancestors, has, alas, been imperfect. This has given occasion for the benign intervention of divine providence, at sundry times and in diverse manners, to discover and enforce its laws through a direct revelation, the doctrines thus delivered, called the revealed or divine law, being found only in the holy scriptures. That its precepts are part of the original law of nature is evident, for they tend in all their consequences to man's felicity, and as expressly declared by God, they are of infinitely more authenticity than the natural law discovered by reason. "Upon these two foundations, the law of nature and the law of revelation, depend all human laws; that is to say, no human laws should be suffered to contradict these."[37] This last is a fundamental point for our analysis, for if human laws cannot be suffered to contradict the moral laws of nature and of revelation, neither can they be suffered to contradict the physical laws of nature, which are prescribed by the very same creator.

If man were to live in a state of nature, Blackstone continues, unconnected with other individuals, only the law of nature and of God would be necessary. Nor could there be any other law, for law always supposes some superior who is to make it, and in a state of nature we are all equal. But man is formed for society, and is neither capable of living alone, nor has the courage to do so. Now, among separate societies or nations, as there is no acknowledged superior, there is only a natural law based upon compacts and treaties called the "law of nations" (*ius gentium*). But each particular district, community or nation is governed by a municipal or civil law of its own that each people establishes for itself (Justinian, *Inst.* 1.2.1.). In compliance with common speech, Blackstone prefers the term "municipal law", and in accordance with his fundamental definition of law, defines it as "a rule of civil conduct prescribed by the supreme power in a state, commanding what is right and prohibiting what is wrong."[38]

To follow the rest of the argument, we advance to Blackstone's general treatment *Of the Laws of England.*[39] The municipal law of England is divided into two kinds, the unwritten or common law and the written or statute law. The latter, however, is always taken to be either declaratory or remedial of the common law, declaratory where it states "what the common law is and ever hath been", remedial where it supplies defects or abridges such superfluities as arise from the general imperfection of all human laws, from change of time and circumstances, from the mistakes of unlearned judges, or from any cause whatever.[40] The common law has been divided into established customs and established rules and maxims, although these are one and the same for the authority of a maxim rests upon showing that it has always been the custom to observe it.[41] How then are these customs or maxims to be known, and by whom is their validity to be determined? The answer is, by the judges in the several courts of justice, the depositaries of the laws, the living oracles, who must decide all cases of doubt according to the law of the land. Their judgments and all proceedings previous thereto are carefully registered and preserved, and frequent recourse is had to them when any critical question arises, in the determination of which former precedents can give light or assistance. For it is an established rule to abide by former precedents, where the same points come again into litigation. So far so good. But what happens when something goes wrong, meaning, not that the law is wrong, but that it has been incorrectly determined?

Yet this rule admits to one exception, where the former determination is most evidently contrary to reason; much more if it be contrary to the divine law. But even in such cases the subsequent judges do not pretend to make a new law, but to vindicate the old one from misrepresentation. For if it be found that the former decision is manifestly absurd or unjust, it is declared, not that such a sentence was *bad law*, but that it was *not law*; that is, that it is not the established custom of the realm, as has been erroneously determined. And hence it is that our lawyers are with justice so copious in their encomiums on the reason of the common law; that they tell us, that the law is the perfection of reason, that it always intends to conform thereto, and that what is not reason is not law.[42]

The point here is that if the decision of a court is contrary to reason or contrary to the divine law, manifestly absurd or unjust, it is *not law*, for what is not reason is not law. And what is true of the evaluation of precedent, is equally true as a court reaches its own decision. And it is here that we can see the grounds of Blackstone's dissent in *Scott* v. *Shepherd*, of his insistence that the injury to the plaintiff was not the immediate act of the defendant, but was consequential, and that the proper action must be, not trespass *vi et armis*, but trespass on the case. It is really quite simple. There is, as it were, a hierarchy of law, of rules of action prescribed by some superior for the obedience of an inferior. The creator, as a being of infinite power, prescribes the laws of inanimate matter and the laws of animate creatures. As a being of infinite wisdom and infinite goodness, he prescribes the laws of human action or conduct, the law of nature including the divine law. These laws can be known through the exercise of reason and, in the case of the last, through direct revelation in holy scripture. Man also makes laws of his own, but these cannot run contrary to reason or to the divine law. Nor can the judges of any court determine the law contrary to reason or to the divine law. Consequently, to descend from the sublime to the mundane, to hold in the case at hand that there was an immediate injury to Scott by Shepherd when the lighted squib, after coming to rest, was twice picked up and thrown again, is a contradiction of Newton's first two laws of motion "to which all moveable bodies must conform," and therefore contrary to reason. It is *not law*. (And it might not be altogether frivolous to add that since the laws of motion were impressed upon matter by the Creator, such a determination would also be contrary to His law.)

EPILOGUE

Blackstone's opinion was not that of the court, by three justices to one, and we presume that poor Scott received his £100. But was the case law? It was certainly cited in the following years, and while in no way rejected, neither was it altogether approved. Curiously, it was cited, not for the distinction of unlawful-lawful between trespass and case, upon which the judgment really rested, but of immediate-consequential, which only Blackstone followed strictly. The subsequent cases, however, were of a kind that was becoming increasingly frequent, collision and running down by vehicles. One may imagine that the young Shepherds were growing up into flash Regency bucks racing about London in their carriages on their way to the fancy in pursuit of the most popular science of the day. And understandably the issue was changing from immediate or consequential injury to willful or negligent misconduct, leading respectively to trespass and case. The court could be slow to grasp this. In *Day* v. *Edwards* (1794)[43] Mr. Edwards "so furiously, negligently, and improperly drove his cart and horse" that he struck with great force and violence upon the carriage called a landaulet of Mr. Day, which was overturned and damaged. Taking negligence as the ground and the injury as consequential, the plaintiff brought an action on the case, but upon a special demurrer to the declaration, the court, citing *Reynolds* v. *Clarke*, ruled that the injury was immediate and the action should have been trespass. Mr. Day's action failed.

Mr. Roome, in a coach with two horses, was being driven about by his servant, who "wilfully" drove upon and against the chaise of Mr. Savignac, which was pulled, forced, and dragged by Mr. Roome's coach and another coach so that it was crushed and broken. *Savignac* v. *Roome* (1794)[44] was an action on the case, and at trial the verdict was for the plaintiff. But Mr. Roome found a very canny barrister, a Mr. Espinasse,[45] who moved at the beginning of the term in arrest of judgment that no action could be maintained against the defendant for a "wilful" act of his servant, accompanied with force, unless done at his command; and if any action could be supported, because of the force it should have been trespass, not case. This was a stroke of genius. Mr. Savignac's attorney, Mr. Bayley, claimed that "wilfully" does not mean that the servant was not acting at the direction of his master, only that the act was not an accident, and he cited cases to the effect that the master is liable for the torts of his servant in any event, and further "because he is guilty of negligence in employing such a servant." To defend the action on the case, he cited Blackstone's dissent in *Scott* that an action of trespass could *not* be maintained if the injury was not the immediate trespass of the defendant himself, so that for an injury arising from the act of a servant, case is the proper action. In reply Mr. Espinasse turned the screw tighter. Citing Lord Holt and 1 *Comm.* 429, he argued that the master is not responsible for the forceful act of a servant for which trespass *vi et armis* lies. And if the action could be supported against the master, it must be trespass, "for in trespass all are principals." Mr. Savignac was boxed in, and Mr. Espinasse proceeded to deliver the final blow. "Either therefore the act complained of in the present case was or was not done by the servant by the defendant's direction: if not, no action can be maintained against the master; if it were, the plaintiff should have brought trespass." Mr. Bayley, his predicament now hopeless, asked leave to amend the declaration to trespass. This the court refused, ruled absolute for arresting the judgment, and Mr. Espinasse doubtless received a handsome honorarium from his client (and perhaps the justices did too).

Not only carriages could collide. In *Ogle* v. *Barnes* (1799),[46] a ship called the "Acteon" was so incautiously, carelessly, negligently, and inexpertly steered that it sailed against a ship called the "Anne," with great damage for which an action on the case was brought. Plaintiff obtained the verdict, and defendant, unfortunately with Mr. Bayley as one of his attorneys, moved in arrest of judgment that the action should have been trespass since the injury was immediate, citing *Scott* for an immediate injury with intervening parties, which has no pertinence to the present case. The court also cited *Scott* for the immediate-consequential distinction, and upheld the verdict on the grounds that the injury was a consequence of negligence.

In *Leame* v. *Bray* (1803),[47] an action of trespass, the plaintiff was being driven by his servant in his curricle drawn by two horses when the defendant drove his single-horse chaise with such force and violence against the curricle that the servant was thrown out upon the ground, and the horses ran away with the curricle so that the plaintiff, for the preservation of his life, jumped out and fractured his collar bone, etc. Evidence at trial showed that the accident happened on a dark night owing to the defendant's driving on the wrong side of the road, although he was in no other way culpable. And it was therefore objected for the defendant that the injury was due to negligence, for which the proper action was case, and the plaintiff was nonsuited. In this leading case the arguments of counsel, with remarks from the bench, are reported at length. The plaintiff's action of trespass was upheld on the grounds that, although the accident was due to the negligence of the defendant, nevertheless it was an immediate injury caused by the force of his chaise, and *Scott* was repeatedly cited for this rule. If the defendant had simply left his chaise in the road and the plaintiff ran into it, it would have been case, just like the log. In the course of the argument of counsel, the following colloquy took place in which Blackstone may be said to have been vindicated.[48]

Justice Lawrence: In *Ogle* v. *Barnes* it did not appear that the force which occasioned the injury was the act of the defendant. But it might have happened from the force of the wind or tide operating at the time directly against the force used by the defendant.
Counsel: Here the continuing motion of the [plaintiff's] carriage was not the immediate act of the defendant.
Lord Ellenborough: If I put in motion a dangerous thing, as if I let loose a dangerous animal, and leave to hazard what may happen, and mischief ensue to any person, I am answerable in trespass.
Counsel: The case of throwing the squib was put upon that ground, but that has never been approved since.
Lord Ellenborough: That case to be sure goes to the limit of the law.

APPENDIX: BLACKSTONE'S OPINION IN SCOTT V. SHEPHERD

The full text of Blackstone's opinion, which is well worth reading and contains the slender thread from which hangs this paper, is given here from his own report, 2 Blackstone Rep. 894–96 (96 *English Reports, Full Reprint Series* 526–28). I have divided the report into paragraphs, expanded the names of reporters, and completed some citations.

Blackstone, J., was of the opinion, that an action of *trespass* did not lie for Scott against Shepherd upon this case. He took the settled distinction to be, that where the injury is *immediate*, an action of *trespass* will lie; where it is only *consequential*, it must be an action on the *case*: *Reynolds* and *Clarke*, Lord Raymond 1401, Strange 634; *Haward* and *Bankes*, Burroughs 1114; *Harker* and *Birkbeck*, Burroughs 1559. The *lawfulness* or *unlawfulness* of the original act is not the criterion; though something of that sort is put into Lord Raymond's mouth in Strange 635, where it can only mean, that if the act then in question, of erecting a spout, had been in itself unlawful, trespass might have lain; but as it was a lawful act, (upon the defendant's own ground), and the injury to the plaintiff only consequential, it must be an action on the case. But this cannot be the general rule; for it was held by the court in the same case, that if I throw a log of timber into the highway, (which is an unlawful act), and another man tumbles over it, and is hurt, an action on the case only lies, it being a *consequential* damage; but if in throwing it I hit another man, he may bring trespass, because it is an *immediate* wrong.

Trespass may sometimes lie for the consequence of a lawful act. If in lopping my own trees a bough accidently falls on my neighbour's ground, and I go thereon to fetch it, trespass lies. This is the case cited from [Y.B.] 6 Edward 4, 7. But then the entry is of itself an immediate wrong. And case will sometimes lie for the consequence of an unlawful act. If by false imprisonment I have a special damage, as if I forfeit my recognizance thereby, I shall have an action on the case; *per* Powel, J., [*Bourden* v. *Alloway*], 11 Modern 180. Yet here the original act [a false imprisonment] was unlawful, and in the nature of trespass. So that *lawful* or *unlawful* is quite out of the case; the solid distinction is between *direct* or *immediate* injuries on the one hand, and *mediate* or *consequential* on the other. And trespass never lay for the latter. If this be so, the only question will be, whether the injury which the plaintiff suffered was *immediate* or *consequential* only; and I hold it to be the latter.

The original act was, as against Yates, a trespass; not as against Ryal or Scott. The tortious act was complete when the squib lay at rest upon Yates's stall. He, or any bystander, had, I allow, a right to protect themselves by removing the squib, but should have taken care to do it in such a manner as not to endamage others. But Shepherd, I think, is not answerable in an action of trespass and assault for the mischief done by the squib in the new motion impressed upon it, and the new direction given it, by either Willis or Ryal; who both were free agents, and acted upon their own judgment. This differs it from the cases put of turning loose a wild beast or a madman. They are only instruments in the hand of the first agent. Nor is it like diverting the course of an enraged ox, or of a stone thrown, or an arrow glancing against a tree; because there the original motion, the *vis impressa*, is continued, though diverted. Here the instrument of mischief was at rest, till a new *impetus* and a new direction are given it, not once only, but by two successive rational agents. But it is said that the act is not complete, nor the squib at rest, till after it is spent or exploded. It certainly has a power of doing fresh mischief, and so has a stone that has been thrown against my windows, and now lies still. Yet if any person gives that stone a new motion, and does farther mischief with it, trespass will not lie for that against the original thrower. No doubt but Yates may maintain trespass against Shepherd. And, according to the doctrine contended for, so may Ryal and Scott. Three actions for one single act! nay, it may be extended *in infinitum*. If a man tosses a football into the street, and, after being kicked about by one hundred people, it at last breaks a tradesman's window, shall he have *trespass* against the man who first produced it? Surely only against the man who gave it that mischievous direction.

But it is said, if Scott has no action against Shepherd, against whom must he seek his remedy? I give no opinion whether *case* would lie against Shepherd for the *consequential* damage; though, as at present advised, I think, upon the circumstances, it would. But I think, in strictness of law, trespass would lie against Ryal, the immediate actor in this unhappy business. Both he and Willis have exceeded the bounds of self-defence, and not used sufficient circumspection in removing the danger from themselves. The throwing it *across* the market-house, instead of brushing it down, or throwing [it] out of the open sides into the street, (if it was not meant to continue the sport, as it is called), was at least an unnecessary and incautious act. Not even menaces from others are sufficient to justify a trespass against a third person; much less a fear of danger to either his goods or his person – nothing but inevitable necessity; *Weaver* and *Ward*, Hobart 134; *Dickenson* and *Watson*, T. Jones 205; *Gilbert* and *Stone* Aleyn 35, Style 72. So in the case put by Brian J., and assented to by Littleton and Cheke, C. J. [Y.B. 6 Edward 4, 7], and relied on in [*Bessey* v. *Olliott & Lambert*] T. Raymond 467, – "If a man assaults me, so that I cannot avoid him, and I lift up my staff to defend myself, and, in lifting it up, undesignedly hit another who is behind me, an action lies by that person against me; and yet I did a lawful act in endeavouring to defend myself." But none of these great lawyers ever thought that trespass would lie, by the person struck, against him who first assaulted the striker.

The cases cited from the Register and Hardres are all of immediate acts, or the direct and inevitable effects of the defendants' immediate acts. And I admit that the defendant is answerable in trespass for all the direct and inevitable effects caused by his own immediate act. – But what is his own immediate act? The throwing of the squib to Yates's stall. Had Yates's goods been burnt, or his person injured, Shepherd must have been responsible in trespass. But he is not responsible for the acts of other men. The subsequent throwing across the market-house by Willis, is neither the act of Shepherd, nor the inevitable effect of it; much less the subsequent throwing by Ryal.

Slater and *Baker* [1 Wilson 362] was first a motion for a new trial *after verdict*. In our case the verdict is suspended till the determination of the Court. And though *after verdict* the Court will not look with eagle's eyes to spy out a variance, yet, when the question is put by the jury upon such a variance, and is made the very point of the cause, the Court will not wink against the light, and say that evidence, which at most is only applicable to an action on the case, will maintain an action of trespass. 2. It was an action on the case that was brought, and the Court held the special case laid to be fully proved. So that the present question could not arise upon that action. 3. The same evidence that will maintain *trespass*, may also frequently maintain *case*, but not *e converso*. Every action of trespass with a '*per quod*' includes an action on the case. I may bring trespass for the immediate injury, and subjoin a '*per quod*' for the consequential damages; – or may bring case for the consequential damages, and pass over the immediate injury, as in the case [*Bourden* v. *Alloway*] from 11 Modern 180, before cited. But if I bring trespass for an immediate injury, and prove at most only a consequential damage, judgment must be for the defendant; *Gates* and *Bailey*, Trinity 6 George 3, 2 Wilson 313. It is said by Lord Raymond, and very justly, in *Reynolds* and *Clarke*, "We must keep up the boundaries of actions, otherwise we shall introduce the utmost confusion." As I therefore think no immediate injury passed from the defendant to the plaintiff, (and without such immediate injury no action of trespass can be maintained), I am of opinion, that in this action judgment ought to be for the defendant.

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NOTES

[1] William Blackstone, *Commentaries on the Law of England*, fifth edition, Oxford, 1773, vol. I, pp. 30, 33 (henceforth cited as e.g. 1 *Comm.* 30). The first edition was published 1765–69. D.J. Boorstein, *The Mysterious Science of the Law*, Cambridge, Mass. 1941, considers the relation of Blackstone's fundamentally conservative "Science of Law" to the new rational philosophies, including natural philosophy, of the eighteenth century, and much else besides. "A thoughtful conservative like Blackstone could not fail to use what he considered to be the best scientific method, and yet he was obliged to apply it in such a way as to prevent its use against accepted beliefs and existing institutions" (p. 11).

[2] 1 *Comm.* 33. It is interesting to note that more than a century and a half earlier, Edward Coke wrote: "Now what arts and sciences are necessary for the knowledge and understanding of these laws [of England]; I say, that seeing these laws do limit, bound and determine of all other human laws, arts, and sciences: I cannot exclude the knowledge of any of them from the professor of these laws, the knowledge of any of them is necessary and profitable. But forasmuch as if a man should spend his whole life in the study of these laws, yet he might still add somewhat to his understanding of them: therefore the Judges of the law in matters of difficulty do use to confer with the learned in that art or science, whose resolution is requisite to the true deciding of the case in question" (3 Rep. pref., ed. G. Wilson, Dublin 1792, p. xix).

[3] 3 *Comm.* 321.

[4] Scott an Infant by his Next Friend, *versus* Shepherd an Infant by his Guardian (1773). 3 Wilson 403. 2 Blackstone Rep. 892. Wilson's report is more complete, including the arguments of counsel, and appears superior in reporting the opinions of at least two of the three other justices, and I have for the most part, although not exclusively, followed it for all but Blackstone's own opinion, which he gives at far greater length in his own report, I presume verbatim from his original written version. Blackstone's opinion is given complete from his own report in the appendix to his paper.

[5] 3 Wilson 404.

[6] 3 *Comm.* 305.

[7] In this very brief summary of the actions of trespass and case, on which the literature is extensive, I am following various parts of Blackstone, *Comm.*, O.W. Holmes, *The Common Law*, Boston 1881, and T.F.T. Plucknett, *A Concise History of the Common Law*, 5th ed., Boston 1956. It should be noted that Holmes writes, not so much an historical analysis, as an argument against strict liability, which seems understandable in 1881.

[8] The leading case is Y.B. 6 Edward IV, Michs. no. 18, f. 7 (1466). The defendant cut thorns from a hedge that fell inadvertently on the property of a neighbor, and entered to retrieve them. While no judgment appears in the report, the discussion of many examples of inadvertent injuries was held to establish strict liability in any trespass to persons or property save in the event of inevitability, e.g. if a storm blows down my tree on to your land. See Holmes 85*ff.*, Plucknett 466. Other cases cited frequently to the same effect are *Weaver* v. *Ward* (1616), Hobart 134, and *Dickenson* v. *Watson* (1682), T. Jones 205, both injuries from the accidental discharge of guns. The cases are reviewed and the principle approved in *Bessey* v. *Lambert & Olliot* (1682), T. Raymond 421, 467.

[9] 3 Wilson 405.

10 3 Wilson 406.
11 Hobart 134.
12 2 Ld. Raymond 1399. 8 Modern 272. 1 Strange 634.
13 1 Strange 635, with which 8 Modern 275 agrees, while in Lord Raymond's own report, 2 Ld. Raymond 1402, the distinction is purely between immediate or consequential injury, not upon whether the initial act was lawful or unlawful. In *Scott* v. *Shepherd* (2 Blackstone Rep. 894) Blackstone says, "The *lawfulness* or *unlawfulness* of the original act is not the criterion; though something of that sort is put into Lord Raymond's mouth in Stra. 635." Rather, it appears that Lord Raymond, having later changed his mind about the grounds of the decision, revised his own report in accordance with the opinions of the other justices. Hence the report represents his later thoughts rather than his opinion on the bench, which seems not altogether correct.
14 1 Strange 636. The log in the highway, a very common example of consequential injury, is from *Fowler* v. *Sanders* (1617), Croke Jac. 446.
15 3 Wilson 407–409.
16 1 Wilson 362, a particularly gruesome case of medical malpractice.
17 3 Wilson 411.
18 Since judgment had been suspended before the final verdict, the plaintiff could still accept a nonsuit and recommence under the proper action, but this would be very inconvenient. 3 *Comm.* 377.
19 3 Wilson 411–13.
20 Sir Michael Foster, *A Report of Some Proceedings on the Commission of Oyer and Terminer and Gaol Delivery for the Trial of the Rebels in the Year 1746 in the County of Surrey, and Other Crown Cases, to which are Added Discourses upon a Few Branches of the Crown Law*, Dublin 1763, p. 261. In the next sentence Foster says that if the act be done heedlessly and incautiously but without mischievous intention, it will be manslaughter, not accidental death, because the act was still unlawful.
21 2 Blackstone Rep. 894–896.
22 2 Blackstone Rep. 895.
23 Newton, *Philosophiae naturalis principia mathmatica*, 3rd ed., ed. by A. Koyré and I.B. Cohen, Cambridge, Mass. 1972, original p. 3 (henceforth cited as Newton).
24 Newton, p. 13.
25 Newton, p. 13.
26 2 Blackstone Rep. 895.
27 2 Blackstone Rep. 895.
28 1 *Comm.* 38.
29 Montesquieu, *De l'esprit des lois* (1748), I, 1.
30 1 *Comm.* 38.
31 Newton, pp. 527–528. To write anything on the General Scholium is surely to bring owls to Athens, and I would never do so were it not pertinent here to consider its use of legal terms.
32 *The Works of Richard Bentley, D.D.*, ed. A. Dyce, vol. 3, London 1838. Sermons vi–viii (pp. 51*ff.*) are dated Oct. to Dec. 1692, just before Newton's four famous letters to Bentley (pp. 203*ff.*) of Dec. 1692 to Feb. 1693, many years before the General Scholium of the second edition of the *Principia* (1713). J.E. McGuire, "Newton on Place, Time and God: An Unpublished Source", *British Journal for the History of Science* **11** (1978), pp. 114*ff.*, transcribes and translates a curious fragment by Newton, apparently from about

1693, on place, time, infinity, eternity, and the nature of God, that may be related to a projected new edition of the *Principia*. In fact it reads much like a sermon, but its purpose is obscure (as is its meaning).

[33] *The Works of the Right Reverend William Warburton D.D. Lord Bishop of Gloucester*, vol. 9, London 1811, particularly Sermon II, *God's Moral Government*, pp. 33*ff.* Blackstone's own religious opinions, at least in relation to the law, can be found in 4 *Comm.* 41*ff.*, *Of Offences against God and Religion*, and they are quite interesting especially with regard to nonconformists and papists (50*ff.*, best read in the first edition before he moderated his language due to several protests). 4 *Comm.* 365*ff.*, *Of the Benefit of Clergy* is also highly recommended.

[34] J. Bentham, *A Comment on the Commentaries* and *A Fragment on Government*, ed. J.H. Burns and H.L.A. Hart, London 1977, p. 275. Bentham's remarks here are an expansion of a draft by John Lind, *ibid.*, p. 352. The unfinished *Comment*, written in 1774–75, was (mercifully) unpublished until 1928. Typical of its author, it is verbose, rancorous, amusing in a rude way, not altogether fair, but pretty much on target. While at Oxford, Bentham was among the thirty to fifty students attending Blackstone's last course of lectures (*ibid.*, p. xx). The *Fragment*, published in 1776, is a criticism of the general consideration of "municipal law" in 1 *Comm.* 47–53. Asked if he would reply to it, Blackstone said, "No, not even if it had been better written" (*DNB* 2, p. 599).

[35] 1 *Comm.* 39–40.

[36] 1 *Comm.* 41.

[37] 1 *Comm.* 42.

[38] 1 *Comm.* 44.

[39] 1 *Comm.* 63*ff.*

[40] 1 *Comm.* 86. This means that there is nothing really new in statute law, merely the recital and occasional clarification of old custom, a point that Coke makes over and over again. Common lawyers frequently spoke disparagingly of statute law, and it was a rule that statutes in derogation of the common law be strictly construed, at times a salutary principle providing some control over the absolute legislative power of Parliament.

[41] 1 *Comm.* 68*ff.* What follows is a close paraphrase.

[42] 1 *Comm.* 69–70. The reference, I believe is to Coke, 1 *Inst.* 56b, "for nothing that is contrary to reason, is consonant to law," and 62a, on customs in diverse manors, "only this incident inseparable every custome must have, viz. that it be consonant to reason; for how long soever it hath continued, if it be against reason it is of no force in law." The words "against reason" are glossed: "This is not to be understood of every unlearned man's reason, but of artificial and legal reason warranted by authority of law: *Lex est summa ratio* (Law is the highest reason)." Blackstone makes much the same point (1 *Comm.* 71) about custom and "artificial reason . . . not quite obvious to every body." Coke could be extravagant in his praise. He called Littleton's *Tenures* "the most perfect and absolute work that ever was written in any human science" by which he means "a work of as absolute perfection in its kind, and as free from error, as any book I have known to be written of any human learning" (1 *Inst.* pref., ed. F. Hargrave, p. xxxvi). The Inns of Court and Chancery he calls "the most famous university for profession of law only, or of any one human science that is in the world, and advanceth itself among all others, *quantum inter viburna cupressus* (as much as the cypress among trees)" (3 Rep. pref., ed. G. Wilson, Dublin 1792, p. xix).

[43] 5 Term Rep. 648.

[44] 6 Term Rep. 125.

[45] I assume this was Isaac 'Espinasse (1758–1834), Barrister at Law of Gray's Inn and author of *A Digest of the Law of Actions and Trials at Nisi Prius* (London 1789 and many later editions, English, Irish, and American) and several volumes of Reports.

[46] 8 Term Rep. 188.

[47] 3 East 593.

[48] 3 East 595–596.

MARGARET SCHABAS

6. FROM POLITICAL ECONOMY TO MARKET MECHANICS: THE JEVONIAN MOMENT IN THE HISTORY OF ECONOMICS*

Since the Enlightenment, economists have traditionally looked to Newtonian physics as the science to emulate. But although the classical economists were clearly aware that their discipline did not match the predictive and explanatory successes of the physical sciences, they did not take specific steps to adopt tools, such as mathematics, that might enhance its credibility as a science. On the contrary, those who addressed such issues, most notably John Stuart Mill, argued that economics used precisely the same "concrete deductive" method as Newtonian physics and was thus already a mature science.[1] The main difficulties encountered in the verification of economic hypotheses were attributed to the complexity of the phenomena and not to any defects in the methods used.

In 1871 William Stanley Jevons challenged Mill and his predecessors by prescribing the extensive mathematization of economic theory. "Economics," he announced at the beginning of his *Theory of Political Economy*, "if it is to be a science at all, must be a mathematical science."[2] Although one can find sporadic attempts to apply mathematics to economic theory prior to 1871 – William Whewell is perhaps the best-known example in England – Jevons was the first to insist that economics must necessarily be treated mathematically.[3] His campaign was a radical departure from previous economic thought and calls for explanation. It is argued here that Jevons's scheme for the mathematization of economic theory was profoundly shaped by his study of contemporaneous developments of the natural sciences, logic, and scientific method, a study which occupied much of his attention during the 1860s and which reached its published form in 1874 in *The Principles of Science*.[4]

According to T.W. Hutchison, Jevons derived the idea of applying mathematics to economic theory from the Benthamite notion of maximizing utility.[5] However attractive an explanation this might sound, it does not conform to the facts. To be sure, the utility theory of value played a significant role in Jevons's approach to economic theory and was in many respects the anchor point for his specific use of the calculus. But Jevons's decision to devise a mathematical treatment of economics

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 235–255.

preceded his commitment to the utility theory of value. His eventual promotion of a hedonic approach to the problem of value, moreover, was not particularly original. As Marian Bowley and R.D.C. Black have shown, and as Jevons himself recognized, the utility theory was well entrenched in the economic literature by the middle of the nineteenth century.[6] The "Jevonian Revolution" constituted not so much a reaction to problems arising within, or to the ideological consequences of, the theory of David Ricardo and J.S. Mill, as an attempt to reinforce the stature of economics as a full-fledged science.[7] The primary factor which led Jevons to convert political economy into the "science" of mathematical economics was independent of the subject matter.[8]

Commentators have often noted that there must be some important connection between Jevons's work on scientific method and his reformulation of economic theory.[9] Although his *Principles of Science* dealt almost exclusively with the natural sciences, Jevons always insisted, both at the start of the *Principles* and throughout the *Theory*, that the social sciences used precisely the same methods as the physical sciences (Jevons, *Principles*, p. xxvii). Indeed, Jevons subscribed to the view, commonplace among Victorian scientists, that "all the sciences meet somewhere. No part of knowledge can stand wholly disconnected from other parts of the universe of thought" (Jevons, *Principles*, p. 154). It seems reasonable to suppose that Jevons's prolonged study of scientific method had some bearing on his lifelong campaign to render economics a mathematical science. But the two attempts made thus far to identify this connection have focused upon the inductive and experimental aspects of Jevons's work in applied economics.[10] My aim here is to identify the links between Jevons's program for mathematical economics and scientific method. More specifically, I will elucidate his efforts to devise a system of market mechanics in direct imitation of the branch of physics commonly known as rational mechanics.

I

Born in Liverpool in 1835, Jevons went to study at University College, London, at the age of fifteen. He soon developed a love for the sciences and subsequently won several prizes in chemistry and botany.[11] He was unable to complete his degree, however, because of financial difficulties. At the end of his second year, he reluctantly accepted a position as an assayer in the Australian mint. Fortunately, the work was not particularly onerous and he continued, during his five-year sojourn in Sydney, to study various subjects: chemistry, meteorology, sociology, and even music theory. A dispute in 1856 on the funding of the Australian railroad sparked his interest in economics and within a year or two, he resolved to return to London, complete his university studies and leave his mark on the world as an economist.

From his first encounters with the literature, notably the works of Adam Smith and J.S. Mill, Jevons discerned the mathematical character of economics. To his sister, for example, he wrote in 1858: "You will perceive that *Economy* scientifically speaking, is a very contracted science; it is in fact a sort of vague mathematics which calculates the causes and effects of man's industry, and shows how it may best be applied" (Jevons, *Papers*, II, 321). After much deliberation, he confessed in the following year to his cousin, the chemist Henry Enfield Roscoe: "You know I am yet in a transition state. I told you, long since, that I intended exchanging the physical for the moral and logical sciences, in which my *forte* will really be found to lie. . . . I wish especially to become a good mathematician, without which nothing, I am convinced, can be thoroughly done. Most of my theories proceed upon a kind of mathematical basis."[12] Thus, even before he returned to London, Jevons had decided to develop a mathematical treatment of economics.

During his three years of study at University College, from 1859 to 1862, Jevons devoted himself to the study of political economy, philosophy, and mathematics. Shortly after completing his Master's degree in the moral sciences, with the Ricardo Prize and M.A. Gold Medal in hand, Jevons submitted a paper, "A Brief Account of a General Mathematical Theory of Political Economy", to the 1862 meetings of the British Association for the Advancement of Science. Although the paper, quite remarkably, contained virtually all of the major theoretical insights of his later *Theory*, it was at the time "received without a word of interest or belief."[13]

Though somewhat discouraged, Jevons decided to change tactics and establish himself in the field of applied economics. The strategy proved successful. His first work, *A Serious Fall in the Value of Gold Ascertained* (1863), gained the attention and admiration of such prominent economists as Walter Bagehot and John Elliott Cairnes. But his second work, *The Coal Question* (1865), brought Jevons more fame than he had ever dreamed of.[14] Mill praised the book in a much-publicized parliamentary debate, and Gladstone purportedly revised his 1866 budget in response to Jevons's pessimistic findings (Jevons, *Papers*, III, 102–103; VII, 11–18). In the same year, Jevons was appointed Codben Professor of Political Economy and Professor of Logic, Mental and Moral Philosophy at Owens College, Manchester. From that point on his career was set. He served as president of the Manchester Statistical Society and of Section F of the British Association. His highest honor came in 1872, when he was elected to the Royal Society.[15] After ten years in Manchester, Jevons returned to London as Professor of Political Economy at University College, a position he held until his early retirement in 1880.

When Jevons at last issued his *Theory of Political Economy* in 1871, it received notices or reviews in most of the prominent English periodicals.[16] Although it was not always well received – Cairnes in particular challenged Jevons's new approach – by the mid-1870s a number of able supporters had joined him in his campaign. One was George Darwin, son of the famous naturalist and, perhaps even more important, a second Wrangler. His attack on Cairnes and defense of mathematical economics in the *Fortnightly Review* of 1875 helped to win support for Jevons's cause, particularly from the reluctant Alfred Marshall.[17]

Reinforcements also came from abroad. Léon Walras, who had independently developed a similar program to recast the foundations of economic theory, offered to join forces. Jevons replied: "I confess that I have always in my own mind attached much importance to this mathematical theory of economy, believing it to be the only basis upon which an ultimate reform of the science of political economy can be founded" (Jevons, *Papers*, IV, 50). A year later, he wrote to Walras: "I have no doubt whatever about the ultimate success of our efforts, but it will take some fighting" (Jevons, *Papers*, IV, 104). Although the "fighting" proved to be relatively mild, it seems to have been effective. By 1881, just one year before his life was cut short by a drowning accident, Jevons reported favorably to Walras: "I am glad to say I think the Math. view of Economics is making much progress in England and is fully recognized by those competent to judge" (Jevons, *Papers*, V, 144). Subsequent economists have tended to agree with Jevons's observation. William Ashley, for example, in a retrospective address of 1907, maintained that "among the diverse lines of thought which converged upon the old orthodoxy for its destruction in 1870–80, that represented by Jevons has for the time had the widest influence."[18]

Jevons wrote some nine books on economic topics, but he regarded his *Principles of Science* as his most important publication (Jevons, *Papers*, III, 250). The work is commonly viewed as a response to the logical treatises of George Boole and J.S. Mill, but insofar as it uses numerous historical examples to illustrate its methodological claims, it is more closely akin to the work of John Herschel and William Whewell. Jevons was clearly not a philosopher of the first rank. In part as a result of his preliminary training as a practical scientist, he had little patience for epistemological problems which might impede what he considered to be the task at hand, namely the defense of science as a legitimate means for the acquisition of knowledge. John Maynard Keynes captured well the main thrust of Jevons's treatise on method when he remarked that "there are few books, so superficial in argument yet suggesting so much truth, as Jevons's *Principles of Science*."[19]

According to Jevons, all forms of reasoning originate in the recognition of identity or similarity. From his insight, that all inference consists in the "substitution of similars," Jevons set out to reform Aristotelian logic. He formulated propositions as algebraic equations and defined deduction as simply the mechanical substitution of identical terms among the premises. In this respect, Jevons's system very much resembled Boole's logic.[20] But, taking issue with Boole, Jevons maintained that all of mathematics could be reduced, via the concept of number, to the more pervasive laws of logic. In this way, Jevons stood Boole on his head: "Boole inverted the true order of proof when he proposed to infer logical truths by algebraic processes" (Jevons, *Principles*, p. 113). Logic, not mathematics, was the most fundamental and hence the most certain branch of knowledge.

Jevons maintained, more specifically, that his logic dealt exclusively with qualitative properties, with the intentionality of terms, and did not bear upon the quantitative properties or extensionally of objects.[21] Quantity, then, was taken to be *the* criterion which demarcated mathematics from its logical foundations. Although natural philosophers had often suggested that quantitative concepts were in some sense mathematical, Jevons made a concerted effort to ground this view in a system of formal logic. Needless to say, his attempt to generate numbers from what he called "logical discrimination" did not win any converts. But in his own mind he was quite confident that he had discovered the true relationship between mathematics and logic.[22]

Jevons's philosophical work belongs strictly to the tradition of British empiricism. He accepted raw sense-data, in conjunction with a few immutable laws of logic, as the starting point of all knowledge. But, in opposition to his immediate predecessors – Herschel, Whewell, and Mill – Jevons reverted back to David Hume's stand on induction. Scientists could never know for certain that the future would resemble the past; it was therefore impossible to identify true causal connections in nature, however much habit might lead us to believe otherwise. Mill's celebrated rules of induction were thus untenable. All scientific laws, Jevons argued, were probabilistic and subject to revision. "Not one of the inductive truths which men have established, or think they have established, is really safe from exception or reversal" (Jevons, *Principles*, p. 238).

Like the majority of his contemporaries, Jevons never seriously questioned the belief that scientific knowledge had progressed. But throughout his writings, he emphasized the view that science had not made nearly as much progress as was commonly thought. No science, he believed, not even Newtonian physics, was perfect or complete. The principle of inertia, the method for the composition of forces, even the very measurement of time and thus of motion, were subject to doubt: "In truth men never can solve the problems fulfilling the complex circumstances of nature. All laws and explanations are in a certain sense hypothetical, and apply exactly to nothing which we can know to exist. . . . When we probe the matter to the bottom physical astronomy is as hypothetical as Euclid's elements."[23]

Jevons was one of the first to state succinctly what we now identify as the hypothetico-deductive method. "In all cases of inductive inference," he maintained, "we must invent hypotheses, until we fall upon some hypothesis which yields deductive results in accordance with experience."[24] It was imperative that scientists continue to recognize that "before we attempt any investigation of facts, we must have correct theoretical notions" (Jevons, *Theory*, p. 22). Nature, nevertheless, could respond objectively to our attempts to fathom its ways. Although the scientist must anticipate the study of nature with appropriate conceptual tools, the scientific method was still primarily an empirical enterprise:

> The investigator begins with facts and ends with them. He uses facts to suggest probable hypotheses; deducing other facts which would happen if a particular hypothesis is true, he proceeds to test the truth of his notion by fresh observations. If any result prove different from what he expects, it leads him to modify or to abandon his hypothesis; but every new fact may give some new suggestion as to the laws in action. Even if the result in any case agrees with his anticipations, he does not regard it as finally confirmatory of

his theory, but proceeds to test the truth of the theory by new deductions and new trials. (Jevons, *Principles*, p. 509).

Throughout the *Principles*, Jevons drew specific attention to the sheer diversity of our encounters with the external world: "Nature is a spectacle continually exhibited to our senses, in which phenomena are mingled in combinations of endless variety and novelty" (Jevons, *Principles*, p. 1). It was therefore necessary that scientists recognize "how hopeless it would be to attempt to treat nature in detail" (Jevons, *Principles*, p. 190). Science must restrict itself to the study of only a fraction of the possible combinations to be found in nature. Furthermore, from any given set of observations, one could formulate an almost endless number of hypotheses. It was thus quite remarkable that science had managed to emerge in the first place, or to continue to grow. This strongly suggested that "the Universe in which we dwell is not the result of chance" (Jevons, *Principles*, p. 2). Although the first step toward a sound theory was a matter of hit or miss, once a breakthrough had been made, nature undoubtedly guided the way: "Chance then must give us the starting point; but one accidental observation well used may lead us to make thousands of observations in an intentional and organized manner, and thus a science may be gradually worked out from the smallest opening" (Jevons, *Principles*, p. 400).

One of the most important heuristic devices in this random search for the laws of nature was the discernment of analogies, either within one branch of science or between different branches. Although the use of analogies was commonplace among nineteenth-century scientists, Jevons went to great lengths to demonstrate the validity of such a practice. Reasoning by analogy was, quite simply, another version of his all-pervading principle of the substitution of similars: "All science . . . arises from the discovery of identity, and analogy is but one name by which we denote the deeper-lying cases of resemblance" (Jevons, *Principles*, p. 629). Jevons's faith in the legitimacy of this practice was definitely reinforced by his conviction that "all the sciences meet somewhere." But he found empirical support as well. Numerous cases in the history of science, for example the researches of René Descartes and Michael Faraday, suggested that "the discovery of an unsuspected analogy between two branches of knowledge has been the starting point for a rapid course of discovery" (Jevons, *Principles*, p. 631). Clearly the most expedient means by which to improve economic theory, for Jevons at least, was to seek analogies between it and the more advanced sciences.

II

Jevons maintained that "every science as it progresses will become gradually more and more quantitative" (Jevons, *Principles*, p. 273). And, given his system of logic, as soon as "quantitative notions enter, . . . the science must be mathematical in nature" (Jevons, *Theory*, p. 7). Classical physics exemplified the fact that the use of a "clear, brief and appropriate system of symbols . . . is almost essential to the expression of those general truths which are the very soul of science" (Jevons, *Principles*, p. 13). Eventually, Jevons claimed, scientific theories would reach such a degree of sophistication that the scientist and mathematician would be indistinguishable from one another: "As a science progresses, its powers of foresight rapidly increases, until the mathematician in his library acquires the power of anticipating nature, and predicting what will happen in circumstances which the eye of man has never examined" (Jevons, *Principles*, p. 526).

By analogy, economics must follow suit. Jevons did not have to establish the view that economics was already a deductive science. On the contrary, the popular press had continually attacked the "dismal science" for being too logical, too prone to lengthy syllogizing. His case was therefore not a difficult one to make. Since economics also had a quantitative complexion, the true character of the discipline would be rendered explicit only if economists would take advantage of mathematical methods: "Economists have long been mathematicians without being aware of the fact. The unfortunate result is that they have generally been bad mathematicians, and their works must fall. Hence the explicit recognition of the mathematical character of the science was an almost necessary condition of any real improvement of the theory" (Jevons, *Theory*, p. xxiii). Since the proper domain of logic was restricted to "pure quality, as apart from quantity", a nonmathematical approach to economics could not adequately treat the quantitative relationships of the market. The assistance of mathematics must be sought: "All economic writers must be mathematical so far as they are scientific at all, because they treat of economic quantities, . . . and all quantities and relations of quantities come within the scope of mathematics" (Jevons, *Theory*, p. xxi). However self-evident or simplistic this might sound to us, it must be remembered that Jevons was one of the first to ascribe logical foundations to mathematics, and to attempt to arrive at criteria that nonetheless distinguished the one branch of knowledge from the other.

Contrary to what one might suppose, Jevons believed that the degree of certainty of a given theory would never be increased through the application of mathematics. Although mathematics might clarify or add rigor to rudimentary scientific ideas, it could never add to the initial truth of the theoretical claims. Indeed, for Jevons, mathematics itself, as it strayed further from strictly logical considerations, became increasingly more dubious. If there was any certainty to be found in mathematics, it was derived from its roots in logic: "The mathematician is only strong and true as long as he is logical, and if number rules the world, it is logic which rules number" (Jevons, *Principles*, p. 154). Thus, primarily as a result of his investigations into the logic of Boole and of Mill, Jevons did not adopt a commonly revered attribute of mathematics.

Jevons's views on the epistemological standing of mathematics were further colored by his acquaintance with non-Euclidean geometry. Jevons was the first person in England to respond to Hermann von Helmholtz's celebrated paper on the subject, and to recognize the element of convention that lay within our choice of a geometry for the physical world. With much perspicacity, he noted that "if, in the course of time, the curvature of our space should be detected, it will not falsify our geometry, but merely necessitate the extension of our books upon the subject."[25] Certainty in geometry was not an empirical question, as Mill had once argued, but rather a function of the internal consistency of the propositions.[26]

Jevons also drew a careful distinction between an exact and a mathematical science. He was aware that as the physical sciences had increased their mathematical profile, they had also tended to become more exact. But this was a historical contingency rather than a necessary connection. In other words, a mathematical science need not have a corresponding degree of empirical precision. The history of science amply suggested that "physicists are, of all men, most bold in developing their mathematical theories in advance of their data." In fact, "had physicists waited until their data were perfectly precise before they brought in the aid of mathematics we would have still been in the age of science which terminated at the time of Galileo" (Jevons, *Theory*, p. 6).

Jevons attempted, consequently, to undermine the belief that mathematics, if introduced to a scientific theory, would imply greater exactitude than was warranted: "Many persons entertain a prejudice against mathematical language, arising out of a confusion between the ideas of a mathematical science and an exact science. They think that we must not pretend to calculate unless we have the precise data which will enable us to obtain a precise answer to our calculations; but, in reality, there is no such thing as an exact science, except in a comparative sense" (Jevons, *Theory*, p. 5). Both certainty and exactitude were viewed as functions of the degree of accordance between fact and theory and were thus assessed after the pure ratiocinative process to which mathematics was applied. In short, mathematics was a powerful theoretical tool to be used in advance of verification.

Although mathematics did not have the virtue of making a science more certain or more exact, it could assist in the discernment of the degree of exactitude; "the approximate character of physical science will be rendered more plain if we consider it from a mathematical point of view" (Jevons, *Principles*, p. 471). This was most evident during the procedure of "quantitative induction," which for Jevons constituted an important part of the scientific method. Insofar as scientists attempt to join together various observations into mathematical functions, all scientific laws, Jevons maintained, were basically of the form: $y = A + Bx + Cx^2 + Dx^3 + Ex^4 + \ldots$ (Jevons, *Principles*, pp. 471–503). The degree of approximation was measured, quite simply, by the number of terms. In principle, every function ought to have an infinite number of terms but, in practice, Jevons acknowledged that scientists had seldom found more than one or two terms. Mathematical functions, accordingly, were viewed as the end product of scientific inquiry. Not only did they reveal the approximate nature of all scientific laws but, by simply connecting phenomena without reference to the operation of causes, they helped to reinforce the view that all scientific knowledge must necessarily fall short of identifying the *versa causa* of nature.

It followed from all of this that economics, in emulating the physical sciences, did not have so far to go as one might have supposed. Throughout his *Principles of Science* Jevons emphasized the view that the natural sciences were not as successful at predicting and explaining the world as was commonly supposed. And, since mathematics was no longer upheld as a source of certainty, its application to the deductive science of economics would commit no injustice. Nor need the use of mathematics, as Mill had suggested, imply that greater precision or exactitude had thereby been achieved.[27] On the contrary, it was only possible to devise a mathematical theory prior to any extensive accumulation of data but, Jevons argued, this had been the very path commonly taken by the leading physical scientists.

III

In his treatment of economic theory, Jevons attempted to establish, wherever possible, algebraic functions between the relevant quantities. His most famous insight was the principle commonly known today as diminishing marginal utility. Assuming that the mind can recognize varying degrees of pleasure and pain, and assuming that net pleasure or utility is generated by the acquisition or consumption of commodities, then there comes a point such that, as the units of a particular commodity are increased, the additional increments or degrees of utility yielded decrease in magnitude. In Jevons's words, "*the degree of utility varies with the quantity of commodity, and ultimately decreases as that quantity increases.*"[28] He next assumed that the units could be treated as infinitesimals which, by rendering the utility function continuous, opened the door for the calculus: "Believing that the quantities with which we deal must be subject to continuous variation, I do not hesitate to use the appropriate branch of mathematical science. . . . The theory consists in applying the differential calculus to the familiar notions of wealth, utility, value, [etc.] . . . As the complete theory of almost every other science involves the use of that calculus, so we cannot have a true theory of Economics without its aid" (Jevons, *Theory*, p. 3).

From his study of physics, Jevons fully appreciated the role that the calculus had played in the development of mechanics. He proposed that an analogous system of mechanics be devised for economics which could then serve as the point of departure for all future research: "As all the physical sciences have their basis more or less obviously in the general principles of mechanics, so all branches and divisions of economic science must be pervaded by certain general principles. It is to the investigation of such principles – to the tracing out of the mechanics of self-interest and utility, that this essay has been devoted. The establishment of such a theory is a necessary preliminary to any drafting of the superstructure of the aggregate science" (Jevons, *Theory*, pp. xvii–xviii). The first step towards the formulation of an economic mechanics had already been taken by the classical economists, namely the treatment of economics as a purely deductive science. In this respect Jevons believed that the methods of economics are "as sure and demonstrative as that of kinematics or statics" (Jevons, *Theory*, p. 21). Economists, moreover, had already identified some of the basic postulates of their science which, like mechanics, were few and simple. There were, however, many additional parallels to be drawn.

Jevons took the phenomena of exchange, the prices and quantities in the marketplace, as the starting point of all economic inquiry. For centuries, moral philosophers had grappled with the problem of exchange-value and had attempted to explain, either directly or indirectly, the system of relative prices in terms of some more fundamental or intrinsic form of value. Jevons's more positivistic approach consisted in taking prices as phenomena, pure and simple, without reference to the more metaphysical concept of value. This was in direct accordance with one of his principal canons of science: "Among the most unquestionable rules of scientific method is that first law that *whatever phenomenon is, is*. We must ignore no existence whatever, . . . if a phenomenon does exist, it demands some kind of explanation" (Jevons, *Principles*, p. 769). It was not the case, then, that prices, due to market disturbances, fluctuate about some natural or absolute value. Rather, every price which appears in the market, no matter how short-lived, "demands some kind of explanation."

According to Jevons, all economic actions stemmed from an imbalance, within any particular mind, of the feelings of pleasure and pain. These mental states were thus the underlying forces of his market mechanics. But it was not necessary, as many of his critics believed, that the feelings of pleasure and pain be measured directly. All that Jevons need assume was that the mind had the capacity to distinguish between two feelings of similar magnitude: "The theory turns upon those critical points where pleasures are nearly, if not quite, equal, . . . that when a man has purchased enough, he would derive equal pleasure from the possession of a small quantity more as he would from the money price of it" (Jevons, *Theory*, p. 13). In the act of exchange, prices were taken to be the manifestation of this mental balancing act. Economic phenomena were thus reducible to mental states and ultimately to the laws of psychology. For Jevons, his "theory presumes to investigate the condition of a mind, and bases upon this investigation the whole of Economics" (Jevons, *Theory*, pp. 14–15).

To justify this explanation of prices by something as subjective and inscrutable as the human mind, Jevons turned to mechanical analogies:

A unit of pleasure or of pain is difficult even to conceive; but it is the amount of these feelings which is continually prompting us to buying and selling, borrowing and lending, . . . producing and consuming; and *it is from the quantitative effects of the feelings that we must estimate their comparative amounts.* We can no more know nor measure gravity in its own nature than we can measure a feeling; but, just as we measure gravity by its effects in the motion of a pendulum, so we may estimate the equality or inequality of feelings by the decisions of the human mind. The will is our pendulum, and its oscillations are minutely registered in the price lists of the markets. (Jevons, *Theory*, pp. 11–12).

In an 1872 letter to Cairnes, Jevons extended these parallels to other aspects of the physical sciences:

The method seems to me exactly analogous to that employed in other theoretical subjects such as that of light, heat, electricity, &c. . . . Scientific men assume those properties as they like, calculate what would happen on such conditions and then by comparison with facts ascertain whether they are correct. They have no means of measuring the properties of the ether except by arguing back from observation. So there is no means of measuring pleasure & pain directly, but as those feelings govern sales and purchases, the prices of the market are those facts from which one may argue back to the intensity of the pleasures concerned. (Jevons, *Papers*, III, 246–247).

Jevons has hereby discerned that the same set of inferential justifications are present in the explanation of the motion of physical bodies as in the motion of prices.

In Jevons's depiction of exchange, two persons barter their respective goods in continuous increments until they reach the point where they are indifferent to obtaining an additional infinitesimal amount of the commodity at the rate of exchange just reached. In this approach, the very act of exchange alters the price continuously. To simplify the analysis, since a complete account would have to explain how a single observed and stable price could emerge from numerous pairs of ongoing exchanges, Jevons proposed that the market be analyzed in a state of static equilibrium, in effect, at the point at which all exchange had just ceased: "The real condition of industry is one of perpetual motion and change. Commodities are being continually manufactured and exchanged and consumed. If we wished to have a complete solution of the problem in all its natural complexity, we should have to treat it as a problem of motion – a problem of dynamics. But it would surely be absurd to attempt the more difficult question when the more easy one is yet so imperfectly within our power. It is only as a purely statical problem that I can venture to treat the action of exchange" (Jevons, *Theory*, p. 93). Jevons was fully aware that "in practice, no market ever long fuflfills the theoretical conditions of equilibrium" (Jevons, *Theory*, p. 111). But, as he had argued at length with respect to the physical sciences, even the most established of them had to invoke hypothetical conditions. Economists, accordingly, were entitled to make some simplifying assumptions.[29]

In order to justify his retreat from confronting the difficult problem of actual market dynamics, Jevons once again drew an analogy to mechanics. His exchange equation, he argued, took precisely the same form as the equation for the law of the lever when analyzed with the aid of the theory of virtual velocities. Jevons believed that he had thereby demonstrated "the mathematical character of the equations of exchange by drawing an exact analogy between them and the equations applying to the equilibrium of the lever" (Jevons, *Theory*, p. xiii). Both the law of exchange and the law of the lever used the concept of an infinitesimal displacement from a position of static equilibrium and thus called for a limited use of the calculus.[30] However persuasive the analogy, Jevons was unable to set up a series of linear equations, differentiate and solve for first and second order conditions. At best, he posited ratios of infinitesimals to suggest a formal connection between prices and the final degrees of utility. Although he was adamant that the "true" theory of economics necessarily required the calculus, he admitted on occasion that he was not "capable of presenting the subject in the concise symbolic style satisfactory to the taste of a practiced mathematician."[31] Subsequent commentators have borne out the view that Jevons had cause to defend his less-than-rigorous mathematics. One of his harsher critics, A.A. Young, maintained that Jevons simply gave a "mathematical garb to results reached by nonmathematical reasoning.[32]

In his effects to emulate rational mechanics, Jevons also sought to expand the domain of economic inquiry to the most general level. His behavioral postulates, for example that "human wants are more or less quickly satiated", were taken to apply across the globe. This was a major departure from the classical theorists, particularly John Stuart Mill, who had insisted that the initial axioms of political economy pertained only to the more advanced industrial nations. Jevons, on the other hand, believed that his postulates were observable in the behavior of Eskimos as much as Londoners. Perhaps the day would come, he once quipped, when their action could be traced "among some of the more intelligent classes of animals."[33] Jevons's attempt to transform economics into a science of universal scope is another instance of his efforts to imitate mechanics: "The theory of the science [of economics] consists of those general laws which are so simple in nature, and so deeply grounded in the constitution of man and the outer world, that they remain the same throughout all those ages. . . . Just as there is a general science of mechanics, so we must have a general science or theory of economy" (Jevons, "Future of Political Economy," p. 198).

As part of his program to render economics into a science of universal scope, Jevons moved certain elements in the classical theory, particularly those with historical dimensions, to the periphery. Thus, in devising a market mechanics, a population, a system of technology, a medium of exchange, and an institutional process for the distribution of income were all assumed to be given at the outset. Once the abstract theory of exchange was worked out, economists could then address the more empirical and hence historically contingent questions of money and banking, for example. Jevons thereby drew a much sharper line between theoretical and applied economics.

The central problem of economics thereby became, in Jevons's hands, a problem of maximizing utility subject to the most general constraints: "*Given, a certain population, with various needs and powers of production, in possession of certain lands and other sources of material: required, the mode of employing their labour which will maximize the utility of the produce*" (Jevons, *Theory*, p. 267). Although Jevons has here formulated economics as a problem of optimization, as a problem which cried out for a solution using the differential calculus, he was unable to solve it mathematically. Jevons had to be content to justify the overriding need for the calculus in part by his assumption that the relevant quantities could be treated in continuous terms and in part by his relatively ad hoc analogy to the law of the lever. To his credit, Jevons acknowledged his limitations: "Two or three correspondents . . . have pointed out that a little manipulation of the symbols, in accordance with the simple rules of the differential calculus, would often give results which I have labouriously argued out. The whole question is one of maxima and minima, the mathematical conditions of which are familiar to mathematicians" (Jevons, *Theory*, p. xiii). Jevons has here glimpsed at a fundamental aspect of economic theory, that the solution to problems really are in terms of extremum properties. His friend and admirer, Francis Ysidro Edgeworth, was the first to iron out these technical difficulties by introducing the Lagrangian analysis of constrained optimization into economics, first in his *New and Old Methods of Ethics* (1877) and then in a book Jevons studied with care, *Mathematical Psychics* 1881).[34]

Although Jevons's actual performance in mathematical economics fell short of his ambitions, his overall approach to economics was nonetheless markedly different from the one taken by his predecessors. Whereas the classical economists had concentrated on long-term growth, on the ways and means to increase the wealth of nations in conjunction with specific institutional traditions and technological innovations, Jevons viewed the economy as a mechanism for the atemporal allocation of scarce resources in accordance with simultaneously reckoned, individual desires. The mere presence of mathematical formulae, moreover, conveyed the impression that economics had been transformed into a universal science without the traditional ties to political institutions. It was not simply by chance that Jevons urged his readers to follow him by renaming the discipline economics rather than political economy.[35] And, once the system of economic mechanics was laid out, it would be possible to construct an "aggregate science" to assimilate the applied topics of banking, tariffs, and taxation. Jevons had thereby introduced fundamental changes to both the methods and scope of economic theory.

IV

Jevons's program for mathematical economics was derived from a mixture of a Victorian faith in the unity of knowledge and an impressive understanding of the limitations of science. As one of the first commentators on the logic of Boole and Mill, Jevons had hit upon a novel approach to the status of mathematical knowledge. More specifically, by establishing a criterion which demarcated mathematics from its roots in logic, and by recognizing, in opposition to Mill, that a mathematical treatment of scientific theories did not entail greater exactitude or certainty, Jevons had discovered the path by which mathematics could secure its rightful place in the "dismal science". Surprisingly, an algebraic analysis of the quantitative relationships of the economy would actually serve to clarify the approximate nature of such knowledge. Moreover, he identified the conceptual point by which to anchor the calculus in economic theory, even though he was unable to pursue this train of thought beyond preliminary formulations.

Jevons's system of logic also gave him an open license to develop historical and formal analogies between economics and the more advanced sciences. Just as physics had become increasingly more quantitative and thus mathematical, so must economics. First, however, it was necessary to construct a general system of economics which, like its counterpart in physics, would be mathematical, reductionist, and universal in scope. Simply put, the prices and quantities of the market place would be reduced to a "calculus of pleasure and pain." Although a complete analysis of the market called for a system of dynamics, Jevons attempted to vindicate his more restricted analysis of exchange in terms of static equilibrium by drawing various analogies to the science of rational mechanics, more specifically, to the principle of virtual velocities as applied to the law of the lever. In fact, it was this analogy which provided Jevons with the means to defend his specific, albeit limited use of the differential calculus.

Jevons's attempts to found economics on a "mechanics of utility and self-interest" did not have a permanent impact. According to Lionel Robbins, economists have since retreated to the less contentious realm of "revealed preferences" and a "logic of choice".[36] But they have continued to utilize mathematical techniques with ever-increasing density. Indeed, judging from the list of Nobel Laureates in economics, it seems that many of our most prominent economists already match Jevons's image of the scientist as "mathematician in his library".[37] For better or for worse, the healthy marriage of mathematics and economics owes much to Jevons's rich understanding of the historical and philosophical dimensions of logic, mathematics, and the natural sciences.

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NOTES

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1 For an appraisal of Mill's views on method, see, J.K. Whitaker, "J.S. Mill's Methodology", *Journal of Political Economy*, 1975, **83**: 1,033–1,050; and Alan Ryan, *J.S. Mill* (London: Routledge and Kegan Paul, 1974).

[2] William Stanley Jevons, *The Theory of Political Economy*, 1st ed. (London: Macmillan and Co., 1871). All quotations are drawn from the posthumous 5th ed. (London: Macmillan and Co., 1957; rpt. ed., New York: Augustus M. Kelly, 1965). Jevons, *Theory*, p. 3.

[3] On Whewell's work, see Pietro Corsi, "The Heritage of Dugald Stewart: Oxford Philosophy and the Method of Political Economy," *Nuncius*, 1987, **2**: 89–144. The most important mathematical economist before Jevons was A.A. Cournot.

[4] William Stanley Jevons, *The Principles of Science*, 1st ed. (London: Macmillan and Co., 1874). All quotations are drawn from the second edition of 1877. During the 1860s Jevons also published several works on logic which were incorporated into the *Principles*. For a list of these, see Harriet A. Jevons, ed., *Letters and Journal of W. Stanley Jevons* (London: Macmillan and Co., 1889).

[5] T.W. Hutchison, *On Revolutions and Progress in Economic Knowledge* (Cambridge: Cambridge University Press, 1978), pp. 86–87. Hutchison also attempts to undermine the significance of the mathematics in Jevons's work: "But the development of mathematical formulations could be held to have been secondary rather than primary in 1871. It was not so much a question for Jevons . . . of championing mathematical formulation *a priori*. . . . Rather the central significance of the marginal concept and the maximizing individual was arrived at first, by Jevons at any rate, and mathematical formulation and the calculus were championed secondarily, . . . as tools for developing and deploying these concepts."

[6] See Marian Bowley, "The Predecessors of Jevons – The Revolution that Wasn't", *Manchester School*, March 1972, **40**: 9–29; and R.D.C. Black, "Jevons, Marginalism and Manchester", *Manchester School*, March 1972, **40**: 5. Jevons recognized, as early as 1860, that his "law of utility has in fact always been assumed by Pol. Econ. under the more complex form and name of the Law of Supply & Demand." R.D.C. Black, gen. ed., *Papers and Correspondence of William Stanley Jevons*, 7 vols. (London: Macmillan Press, 1972–1981), II, 410. Hereafter cited in the text as Jevons, *Papers*. Jevons's developed analysis of exchange demonstrated not only "the final equivalence of labour and utility," but also "the well-known law, . . . that value is proportional to the cost of production." Jevons, *Theory*, pp. 177, 186.

[7] For representative statements of the internalist position, see Hutchison, *On Revolutions*, chaps. 3–4; and Maurice Dobb, *Theories of the Value and Distribution Since Adam Smith* (Cambridge: Cambridge University Press, 1973), chap. 7.

[8] In order to carry out such innovations, one would have had to have studied some mathematics first. Jevons, however, was by no means the first British economist to have acquired these skills. Thomas Malthus, Henry Fawcett, and Jacob Waley, all professors of political economy, held degrees in mathematics. Even Mill had studied some calculus at the university level. A knowledge of mathematics, while a necessary condition for the transformation under consideration here, was clearly not, at least in nineteenth-century Britain, a sufficient one.

[9] See, for example, Philip Henry Wicksteed, *The Common Sense of Political Economy*, Lionel Robbins, ed. (London: George Routledge & Sons, 1935), II, 809; and R.D.C. Black, "Intoduction," W.S. Jevons, *Theory of Political Economy*, (Harmondsworth: Penguin, 1970), pp. 13–14.

[10] According to one study, "the most striking instance of the impact of Jevons's results in scientific method on his economics is to be found in the introduction to his paper on

'The Solar Period and the Price of Corn'". Barbara MacLennan, "Jevons's Philosophy of Science", *Manchester School*, March 1972, **40**: 62–63. The other paper, while acknowledging that Jevons's system of logic must have played a part in his theory of economics, emphasizes the role of statistics and experimentation in Jevons's work on monetary economics and social reform. Wolfe Mays, "Jevons's Conception of Scientific Method", *Manchester School*, September 1962, **30**: 233–249.

[11] Jevons actually started at the Junior School of University College before spending two years at the College proper, where he had the good fortune to study with one of the leading mathematicians of the time, Augustus De Morgan. Although Jevons tended to deprecate his own abilities in the subject, he succeeded in placing fourth on the lower senior-class examination in mathematics in 1853. See R.D.C. Black, "Jevons, Bentham, and De Morgan", *Economica*, May 1972, **39**: 119–134.

[12] Harriet A. Jevons, *Letters and Journal*, pp. 118–119. Another letter from Roscoe urged Jevons to continue his work in the physical sciences "in spite of . . . taking to the Mathematics of Society", Jevons, *Papers*, II, 355.

[13] Jevons, *Papers*, I, 188. The paper was subsequently published in the *Journal of the Statistical Society of London*, 1866, **29**. One of the first to correspond with Jevons on the piece, in 1868, was Fleeming Jenkin. See Jevons, *Papers*, III, 166–178.

[14] To his sister, Jevons wrote in 1866, "excuse my being a little two full of myself at present. It is hard even for me to feel the full meaning of such sudden and complete success. If I had worked ten or twenty years longer, I might have been glad to have got the result I already have got", Jevons, *Papers*, III, 97.

[15] Jevons was appointed primarily for his work in statistics and the natural sciences. John Maynard Keynes, in his *Essays in Biography* (London: Rupert Hart-Davis, 1951), made much of the fact that Jevons was the first economist to be elected to the Royal Society since Sir William Petty (p. 258). In fact, Thomas Robert Malthus was also a fellow.

[16] Brief notices were printed in the *Anthenaeum*, the *British Quarterly Review*, the *Westmininster Review*, and the Manchester and London newspapers. Full reviews appeared in the *Saturday Review* (anonymous), the *Academy* (Alfred Marshall), and the *Fortnightly Review* (J.E. Cairnes). These have been reprinted in Jevons, *Papers*, VII, 141–152. Apart from the popular magazines, only the *Contemporary Review* and *Quarterly Review* overlooked the *Theory*.

[17] George Darwin, "The Theory of Exchange Value," *Fortnightly Review*, February 1875, **23**: 244–253. Jevons received some letters, including one from Alfred Marshall, which endorsed Darwin's rebuttal of Cairnes (Jevons, *Papers*, IV, 100).

[18] William Ashley, "A Survey of the Past History and Present Position of Political Economy," in R.L. Smyth, ed., *Essays in Economic Method* (London: Gerald Duckworth, 1962), p. 232. Schumpeter has noted that the majority of major economists in the period 1870–1914 were acquainted with or actually practiced applied mathematics. See Joseph A. Schumpeter, *A History of Economic Analysis* (New York: Oxford University Press, 1954), p. 956. I have reinforced the accuracy of Jevons's observations in "Alfred Marshall, W. Stanley Jevons, and the Mathematization of Economics", *Isis*, March 1989, **80**: 60–73.

[19] John Maynard Keynes, A *Treatise on Probability* (London: Macmillan, 1921), p. 273. A contemporary critic made a similar judgment: "[A]s a methodologist he [Jevons] has fairly outstripped predecessors as great as Herschel, Whewell, and Mill. . . . [But]

Jevons does not equal either Whewell or Mill in philosophical grasp. . . . He appears least at ease when he touches upon questions properly philosophical." See George Croom Robertson, "Mr. Jevons's Formal Logic", *Mind*, 1876, **1**: 207.

[20] See Wolfe Mays and D.P. Henry, "Jevons and Logic", *Mind*, October 1953, **62**: 484–505; and William Kneale and Martha Kneale, *The Development of Logic* (London: Oxford University Press, 1962), pp. 420–422.

[21] For his explanation of this distinction, see Jevons, *Principles*, p. 26. C.I. Lewis maintains that subsequent work has rendered the distinction superfluous; Jevons's system of logic would not be altered were extension to be incorporated. See C.I. Lewis. *A Survey of Symbolic Logic* (1960; abridged ed., New York: Dover, 1960), p. 73.

[22] Frege found Jevons's work meritorious. See Gottlob Frege, *The Foundations of Arithmetic: A Logico-Mathematical Enquiry into the Concept of Number*, L.J. Austin, trans. (Evanston, Ill.: Northwestern University Press, 1980), pp. 46–58.

[23] Jevons, *Principles*, p. 458. Jevons identified several of the questionable or non-testable assumptions in Newtonian physics. Even time, he noted, is measured purely according to convention: "[I]nasmuch as the measure of motion involves time, and the measure of time involves motion, there must be ultimately an assumption" (Jevons, *Principles*, p. 309).

[24] Jevons, *Principles*, p. 228. According to one scholar, "the account of scientific method which became recognized as the official alternative and rival to Mill's was not Whewell's but Stanley Jevons." See Peter B. Medawar, *The Art of the Soluble* (London: Methuen, 1967), p. 149. For more detailed studies of Jevons's contributions to the philosophy of science, see John V. Strong, "The Infinite Ballot Box of Nature: De Morgan, Boole, and Jevons on Probability and the Logic of Induction", in *Proceedings of the Philosophy of Science Association*, 1976, **1**: 197–211; Larry Laudan, "A Note on Induction and Probability in the Nineteenth Century", in *Science and Hypothesis* (Dordrecht: Reidel, 1981); and Edward H. Madden, "W.S. Jevons on Induction and Probability", in *Theories of Scientific Mehtod* (Seattle: University of Washington Press, 1966), pp. 233–247.

[25] See W.S. Jevons, "Helmholtz on the Axioms of Geometry", *Nature*, April–October 1871, **4**: 481–482. Also see Joan L. Richards, *Mathematical Visions: The Pursuit of Geometry in Victorian England*, (San Diego: Academic Press, 1988).

[26] Jevons demolishes Mill's views on the empirical foundations of geometry in his "John Stuart Mill's Philosophy Tested", reprinted in William Stanley Jevons, *Pure Logic and Other Minor Works*, Robert Adamson and Harriet A. Jevons, eds. (London: Macmillan and Co., 1890).

[27] Mill also believed that the use of algebra in economics would suggest that precise and universal laws had thereby been achieved. See Neil B. de Marchi, "Mill and Cairnes and the Emergence of Marginalism in England", *History of Political Economy*, Fall 1972, **4**: 344–363; and Margaret Schabas, "Some Reactions to Jevons's Program for Mathematical Economics: The Case of Cairnes and Mill", *History of Political Economy*, Fall 1985, **17**: 337–353.

[28] Jevons, *Theory*, p. 53, Jevons set out in algebraic form that both u and $\mathrm{d}u/\mathrm{d}x$ are functions of the commodity x, where u stands for utility. But he chose to represent the more fundamental relationship of the eventual decrease of the degree of utility geometrically rather than with the simple inequality: $\mathrm{d}^2u/\mathrm{d}x^2 \leq 0$.

[29] Again, he drew a specific analogy to mechanics: "It is much more easy to deter-

mine the point at which a pendulum will come to rest than to calculate the velocity at which it will move when displaced from that point of rest. Just so, it is a far more easy task to lay down the conditions under which trade is completed and interchange ceases, than to attempt to ascertain at what rate trade will go on when equilibrium is not attained" (Jevons, *Theory*, p. 94).

30 Joseph-Louis Lagrange, in his *Méchanique Analytique* (1788), paid tribute to this principle, now known as the principle of virtual work: $\Sigma m_i \vec{v}_i d\vec{v}_i = 0$. See Jevons, *Theory*, pp. 102–106 for its application to economics.

31 Jevons, *Theory*, p. xiii. Jevons excused his limited facility with mathematics with the remark that he was writing, not for mathematicians, "but as an economist wishing to convince other economists that their science can only be satisfactorily treated on an explicitly mathematical basis" (Jevons, *Theory*, pp. xiii–xiv).

32 Allyn A. Young, "Jevons's Theory of Political Economy", in *Economic Problems New and Old* (Boston: Houghton Mifflin, 1927), p. 230.

33 William Stanley Jevons, "The Future of Political Economy", in *The Principles of Economics and Other Papers*, Henry Higgs, ed., (London: Macmillan and Co., 1905; rpt. ed., New York: Augustus M. Kelly, 1965), p. 197. We know from a letter to his wife that Jevons partly intended the claim about animals as a joke. See Jevons, *Papers*, IV, 182.

34 See John Creedy, *Edgeworth and the Development of Neoclassical Economics* (Oxford: Basil Blackwell Ltd., 1986), pp. 34–37.

35 In the second edition of the *Theory*, Jevons consistently replaced every occurrence of "political economy" with "economics", with the sole exception of the title itself. See *Theory*, pp. xiv–xv. Alfred Marshall's *Principles of Economics* (1890) established the modern appellation.

36 See Lionel Robbins, "The Place of Jevons in the History of Economic Thought", *Manchester School* 1936, **7**: 6–7.

37 See Joel Franklin, "Mathematical Methods in Economics", *American Mathematical Monthly*, April 1983, **90**: 229–244. Of the twelve prizes given in the years 1969–1981, seven were for work which Franklin deems primarily mathematical. Two more recent awards, to George Stigler and Gerard Debreu, fall under the same category.

GIULIANO PANCALDI

7. THE TECHNOLOGY OF NATURE: MARX'S THOUGHTS ON DARWIN

1. INTRODUCTION

There is impressive, prima facie evidence supporting the view that Karl Marx's relationship to Charles Darwin should be regarded as a strong case in the history of the interaction between the natural sciences and the social sciences. Consider for example Marx's straightforward declaration according to which Darwin's work contained "a scientific basis for the historic class struggle".[1] Or take Friedrich Engels' emphatic statement at Marx's graveside: "Just as Darwin discovered the law of development of organic nature, so Marx discovered the law of development of human history".[2] Yet, there is also impressive evidence showing that Marx was unwilling to base his social views direct on biology, and that he thoroughly rejected attempts aimed at explaining social traits away with the natural conditions supporting individuals and societies. Besides, Marx liked to stress discontinuity rather than continuity between the animal world and human societies. So, he pointed out that population dynamics followed different laws among animals and in human societies, and he contrasted animals' instinctive behavior with man's goal-directed work. It seems thus fair to say that Marx's relationship to Darwin is a strong case, above all, in that it shows how entangled the interaction between the natural sciences and the social sciences can be.

In the present paper I shall suggest that Marx's attitude towards Darwin should be viewed as the result of two different though connected circumstances. The first was Marx's and Engels' interest in presenting Marx's own social and economic theory as a scientific theory, comparable to evolution theories in biology and thus sharing their scientific status. This circumstance had to do with ideology as well as with the interaction between the life and the social sciences. The second circumstance was that, whatever the motives – ideological or philosophical – bringing Marx towards Darwin, Marx also developed a penetrating interpretation of Darwin's theory. Marx's interpretation, still valuable and seldom considered by historians of science, viewed Darwin's

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 257–274.

theory as a theory about the "technology of nature". I shall argue that this interpretation of Darwin's theory on the part of Marx is itself an interesting case in the history of the interaction between the natural sciences and the social sciences.

The relations between Charles Darwin and Karl Marx have been dealt with by historians along few identifiable lines of approach. A first group of historians have set themselves the modest but useful task of focusing on the actual personal contacts between Darwin and Marx. In recent years they have succeeded in separating truth from myth in a much-discussed story.[3] As a result of this line of research it is now agreed that the direct personal contacts between Darwin and Marx did not amount to much: Marx sent Darwin a copy of the second German edition of *Capital*, with a dedication in which he declared himself Darwin's "sincere admirer",[4] and Darwin answered – after a four-month delay – with a polite but unencouraging letter.[5]

A second group of commentators have undertaken the more ambitious task of comparing Darwin's and Marx's achievements in their respective fields, natural history and social theory. The germs of this approach, as already mentioned, can be traced back to Marx and Engels themselves. Since then, conjectures have proliferated regarding the existence of some kind of parallelism between Darwin's and Marx's theories. Few studies so far have, however, managed to set the issue of parallelism in proper historical terms, that is to say, within the wider issue of the diffusion – both joint and separate – which Darwinism and Marxism enjoyed in different countries.[6] More often the issue of parallelism has been debated in a general, philosophical perspective. This is usually the stand adopted by authors interested in arguing for or against the legitimacy of such parallelism.[7] These authors have often insisted that no final assessment of the relationships between Darwin and Marx should avoid tackling such complex questions as the interaction of evolutionism, historicism, and Hegelianism in nineteenth-century thought. It seems fair to say that while this approach has helped remind historians of the peculiar complexity of the Darwin-Marx relationship, it has done little to set the question on firmer grounds.

In what follows I shall deal with the controversial issue of parallelism between Darwin's and Marx's theories from a narrower but perhaps more workable perspective. I shall first survey Marx's and Engels' reactions to the *Origin* and other related books on biological evolution. Though parts of its are well known, the story is still instructive. It helps us understand the kind of interests at work in Marx's and Engels' readings of Darwin, as well as the ambiguities affecting their attitudes. I shall then test the thesis of parallelism by considering Marx's treatment in *Capital* of two issues well suited to revealing convergences or divergences between Darwin's and Marx's theories, i.e., the role played by physical environment in nature and society, and the laws regulating human and animal populations. The treatment of these and some minor issues can be expected to shed light on an aspect of Marx's reflections on Darwin some commentators[8] have signaled as perhaps the most valuable and original; namely, Marx's view of Darwin's theory as a theory concerning the "history of natural technology".

2. MARX, ENGELS, AND THE "ORIGIN"

Marx read or at least went through the *Origin* twice in the first three years after its publication. He made a detailed analysis of the relations between Darwin and Malthus, and discussed the comparative merits of Darwin's theory and other theories being contributed to the field of evolutionary natural history during the 1860s. In 1867 he discussed with Engels which affinities between the *Origin* and *Capital* seemed appropriate to point out to potential readers of his work. He consulted Darwin's book again while drafting some notes for the second German edition of Capital.[9] Finally, he watched the diffusion of Darwinism in German and Britain during the 1870s, condemning the way Darwin's theory was being appropriated by the social sciences. Marx's interest in Darwin was thus far from episodic.

Of the well-known pair of revolutionary activists it was Engels who read the *Origin* first, in December 1859, a few days after it came out; it was probably also Engels who urged Marx to read it. Writing about it to Marx on December 11 or 12, 1859, Engels expressed his admiration for Darwin's work and called attention to two points. The first was the defeat of the teleological view of nature that, in Engels' opinion, the *Origin* had achieved. The second was what Engels described as the "grandiose" attempt carried out by Darwin to "demonstrate a historical development of nature".[10] Engels thus appears to have been struck by two different aspects of Darwin's work. His remark on teleology seems to concern the particular explanation suggested by Darwin for evolutionary processes, an explanation which a considerable number of his contemporaries thought would put an end to the traditional view of design in nature.[11] The second remark concerned, rather, the historical dimension of nature postulated by Darwin's theory, and by a number of transformist theories that had been put forward in geology and natural history from the eighteenth century. Darwin's work was thus being evaluated at two somewhat different levels – as an explanation of evolutionary processes and as offering new and convincing evidence of the historical dimension of nature – levels which were mingled in Engels' mind as in that of many a contemporary commentator. Historians of Darwinism are familiar with examples of this two-level evaluation of Darwin's theory.[12] In Marx's and Engels' reflections on Darwin, as we shall see, it deserves special attention because of its particular consequences.

Marx read the *Origin* for the first time in December 1860, one year after Engels. He was greatly impressed by it. Writing to Engels he commented: "Although it is developed in the crude English style, this is the book which contains the basis [*Grundlage*] in natural history for our view".[13] The disparaging phrase on the "crude English" style is easily understood in view of Marx's familiarity with the subtleties of German idealistic philosophy, for which there was no room in Darwin's work. As for the second part of Marx's statement, it is difficult to imagine a more concise and more intriguing remark. Its brevity should be an invitation to refrain from attributing any definite interpretation to it. Still, Marx made a similar comment, with additions, in an already mentioned letter to Ferdinand Lassalle in January 1861: "Darwin's book is very significant, and I like it as a scientific basis [*Unterlage*] for the historic class struggle.[14] That adjunct on struggle does not seem to occur again in Marx's writings in connection with the issue of a scientific foundation for his own views. One comment, however, seems safe. If Marx does not tell here in what precise sense the *Origin* might offer "the basis in natural history" for his own views, his statements do indicate that he was interested in looking for such a biological basis of his social theory.

That Darwin's work incorporated concepts which naturalists had borrowed from economists,[15] did not, of course, escape Marx's attention. Having returned to the *Origin*, in June 1862 he wrote to Engels: "Darwin recognizes among beasts and plants his English society with its division of labour, competition, the opening up of new markets, 'inventions', and the Malthusian 'struggle for existence.' It is Hobbes's *Bellum omnium contra omnes*. This recalls Hegel's *Phenomenology*, where civil society is described as a spiritual animal kingdom, while in Darwin the animal kingdom figures as a civil society".[16]

Thus, even before the striking developments of social Darwinism, Marx perceived the important place occupied by the viewpoint of the "economy of nature" in the *Origin*.[17] This led him to poke fun at Darwin's tendency to see the animal world as if it were a bourgeois society; yet, as we shall see, Marx also noted the divergences between Darwin and Malthus, and occasionally he was himself tempted to search in natural history for a basis for his own economic and social views.

3. THE TEMPTATIONS OF ENVIRONMENTALISM

That Marx had some recurrent idea of linking his view to prevailing biological theories emerges in detail from his correspondence with Engels, where the two discussed a work published in 1865 by a little-known French author, Pierre Trémaux. The lengthy title of Trémaux's book began with the words *Origine et transformation de l'homme et des autres êtres*, and ended with the author's declared intention to deal with such issues as *la base des sciences naturelles, historiques, politiques, etc.*[18]

Marx initially found Trémaux's work "of great importance," and constituting a "*very remarkable* advance on Darwin"[19] in ways which included its "historical and political applications." Marx expressed his high regard for Trémaux to both Engels and Ludwig Kugelmann, in terms that leave no doubts as to his deep committment. Just what, then, were the sources of Trémaux's appeal for Marx?

To a lay reader like Marx, Trémaux's book may well have appeared easier and more straightforward than Darwin's *Origin* in its support of a general evolutionary view of nature and society. Trémaux offered a sweeping overview of the principal theories of species under debate in the early 1860s. A traveller with little or no training in any of the special branches of natural history, except perhaps geography, Trémaux's declared objective was to suggest an easy solution to the "mystery of mysteries" that had troubled naturalists from Lamarck to Darwin: the problem of species. His proposal was a rather simplistic re-statement of the old speculations concerning the *direct* action of physical environment on living beings. Trémaux claimed that geography and geology revealed a substantial correspondence between the physical features of the soil and the organisms living on it. He admitted that the idea was as old as Herodotus, and so common as to be shared by any gardener. Trémaux actually had no new evidence at all concerning the mechanisms through which the "soil" acted, but he tried to heighten the appeal of his solution by presenting it as a convenient compromise between the conflicting theories of species current in France in the early 1860s.[20]

Although in communicating his ideas to the Académie des sciences,[21] meeting under the presidency of its perpetual secretary Marie-Jean-Pierre Flourens, Trémaux went to some pains to adopt some of the arguments supporting the fixity of species, he clearly sided with the evolutionists. Of these he paid special hommage to Isidore Geoffroy Saint-Hilaire, mentioning Lamarck only in passing, and criticizing Darwin rather severely. He denied that Darwin's concept of the struggle for life could perform any constructive role in the transformation of species, and maintained that struggle had equivalent effects on well and badly adapted individuals. Above all, he criticized Darwin for having minimized the direct action of physical environment, and for having cast doubts on the certainty of progress.[22]

In their political and social applications, Trémaux's ideas amounted to the simple and effective sentence he liked to repeat often in his book, once even putting it in capital letters: "Hors des grandes lois de la nature, les projects des hommes ne sont que calamités!".[23]

The dictum had great impact on Marx, who emphasized it in his letter to Engels. Judging from Marx's comments on Trémaux, what impressed him most and led him for some time to prefer Trémaux to Darwin[24] was precisely the importance attributed to the direct action of physical environment. The tradition of environmentalism still had deep roots in medicine, anthropology, and the social sciences in the mid-nineteenth century and later. One British example of this tradition is Thomas Buckle's *History of civilization in England*, highly praised by John Stuart Mill in his *System of logic*.[25] Buckle included climate, soil, and nutrition among the main factors acting on human societies. It would appear that for Marx as for many a social theorist of his time, environmentalism and the certainty of progress sanctioned by some versions of biological transformism had greater appeal than the search for an adequate explanation of evolutionary mechanisms conducted by Darwin. On this point, however, Engels turns out to have been more cautious, and more penetrating than Marx.

Induced to read Trémaux by his friend's enthusiastic comments, Engels received a thoroughly unfavourable impression. Although he gave Trémaux "credit for attaching a greater importance [than Darwin] to the influence of the 'soil' on race formation and consequently on species formation," he complained to Marx

Darwin and others have never denied the influence of the soil, and if they have not attached special importance to it, it is because they knew nothing of *how* soil acts . . . Tr[émaux] does not know much more about it . . . And if he further declares that the influence of more recent or more ancient soil, modified through crossing, is the only cause of change in organic species, or races, I see no reason for following the writer that far, and I find a mass of objections against it.[26]

Marx was only partly convinced by Engels' vigorous arguments; as already mentioned, soon after reading them he praised Trémaux again to another correspondent. However, he added few cautionary words when expressing his opinion of Darwin's and Trémaux's comparative merits,[27] and, as far as we know, Trémaux's name never occurred again in Marx's later works and correspondence, while in 1873 he addressed a copy of the second edition of *Capital* to Darwin, with the flattering dedication already mentioned.[28]

Besides casting light on the temptations of environmentalism, the Trémaux episode suggests that Marx's interest in Darwin was in a sense derivative. It reflected his interest in the general issue of the historical dimension of nature, and, more especially, in the role that physical environment played in shaping human societies. From this perspective Darwin's work could easily be put on a par with other books on biological evolution being published in the central decades of the century, whereas the theory of natural selection may have appeared irrelevant, or unnecessarily abstruse. Some of Marx's further comments on Darwinian issues, however, testify that he was at times capable of a less superficial appreciation of Darwin's work.

4. HUMAN AND ANIMAL POPULATIONS

Marx managed to resist the temptations of environmentalism in the first book of *Capital*, which he was working on in 1865–1866, when he exchanged ideas with Engels on Trémaux. Continuity and discontinuity between the natural world and human societies are frequent themes for reflection in *Capital*. Marx's view was that there exists a "natural basis" exerting an influence on economic life as, for example, on work productivity. This is partly a question of man's nature, and partly one of the natural resources available to a given society. Yet, the role of these factors is limited compared with that of the economic factors central to

Marx's analysis. As he puts it, "favourable natural conditions can provide themselves only the possibility, never the reality of surplus labour, nor, accordingly, the reality of surplus-value and a surplus product".[29] It would therefore be a mistake, in Marx's opinion, to explain the characteristics of a society taking only its natural conditions into account; or, in terms of an example, to seek in nature, rather than in the introduction of the capitalistic mode of production, the reason why an inhabitant of the Asiatic archipelago, who once was capable of securing his week's food in just a single working day, may be forced to work six days a week.[30]

Nor can natural conditions satisfactorily explain important phenomena like the dynamics of human populations. Whereas Malthus had presented his population law as a universal natural law, Marx stresses that human demographic trends are in fact subject to social conditioning. If a general law of population exists, Marx argues, it is "only for plants and animals, and even then only in the absence of any historical intervention by man"[31].

This last remark is relevant to the relations between Darwin and Marx we are concerned with here. Although Marx does not mention Darwin's name when discussing this point in *Capital*, we know he had recognized the importance of Malthus' arguments for Darwin. In his 1862 comments to Engels on the *Origin*, he points out in an amused tone the inconsistencies he feels Darwin runs into

> when he says he is applying the "Malthusian" theory *also* to plants and animals, as if with Mr. Malthus the whole point were not that he does *not* apply the theory to plants and animals, but only to human beings and with geometrical progression, as opposed to plants and animals [which are assumed to develop only with arithmetical progression].[32]

Despite the criticism, Marx here seems to credit the naturalists – perhaps Darwin himself – with the demonstration of a population law valid for plants and animals. After all, the *Origin* was the best-known natural history book dealing extensively with the subject. Marx thus noted that Darwin had partly borrowed the idea from the economists, but Marx himself was unwilling to grant the economists' population law the status of a natural law. The rationale for this was Marx's inclination to admit to a radical distinction between the laws regulating human and animal populations. In fact only once, in a sarcastic vein, did he write of a "principle of natural selection" acting between agricultural and industrial populations,[33] the same sarcasm he displayed elsewhere when recalling Hegel's description of civil society as a "spiritual animal kingdom".[34]

Marx's insistence on the discontinuity between the vicissitudes of human and animal populations left little room for possible convergences, in this respect, between Marx's and Darwin's theories. Yet Marx's comments on Malthus and Darwin were perceptive, and reveal a deeper understanding of Darwin's theory of evolution that some of his other remarks suggest. His reflections on Darwin and the technology of nature confirm this.

5. THE HISTORY OF NATURAL TECHNOLOGY

In a note added to the second German edition of Capital (1872), Marx describes Darwin's study of the development of living forms as an inquiry into "the history of natural technology, i.e. the formation of the organs of plants and animals, which serve as the instruments of production for sustaining their life." This characterization of Darwin's theory was the first step in Marx's own original proposal to develop a "critical history of [human] technology," that is to say, a "history of the productive organs of man in society, of organs [machines, inventions, etc.] that are the material basis of every particular organization of society".[35] Elsewhere in *Capital* he remarks, "Relics of bygone instruments of labour possess the same importance for the investigation of extinct economic formations of society as do fossil bones for the determination of extinct species of animals".[36]

These remarks suggest a few significant comments. Marx is dealing here with the core of Darwin's theory, i.e. the theory of natural selection, rather than with the general evolutionary issues he seems to pay attention to elsewhere. Besides, Marx's comments imply that, in his view, the themes inspired by economics in the *Origin* extended beyond the controversial issue of Malthusian population laws; they included as well what Marx considered Darwin's "technological" approach to the study of plant and animal life. In another note in *Capital* Marx mentions, as an example of that approach, a passage where Darwin discusses the specialization of organs in relation to special functions:

> . . . as long as the same part has to perform diversified work, we can perhaps see why it should remain variable, that is, why natural selection should have preserved or rejected each little deviation of form less carefully than when the part has to serve for one special

purpose alone. In the same way that a knife which has to cut all sorts of things may be of almost any shape; whilst a tool for some particular object had better be of some particular shape.[37]

Darwin scholars have generally neglected the techological images and metaphors present in Darwin's writings.[38] Suffice it to recall here one such image which Darwin utilized time and again in the earlier versions of his work on species. Judging from his repeated efforts to perfect it through successive versions, he must have thought the imagine conveyed an effective view of his theory. In its fullest form, it reads as follows:

Nature may be compared to a surface covered with ten-thousand sharp wedges, many of the same shape & many of different shapes representing different species, all packed closely together & all driven in by incessant blows: the blows being far severer at one time than at another; sometimes a wedge of one form & sometimes another being struck; the one driven deeply in forcing out others; with the jar & shock often transmitted very far to other wedges in many lines of direction: beneath the surface we may suppose that there lies a hard layer, fluctuating in its level, & which my represent the minimum amount of food required by each living being, & which layer will be impenetrable by the sharpest wedge.[39]

It should be noted, incidentally, how different Darwin's image of the ten thousand wedges driven in by the irregular blows of a huge hammer was from the then still prevailing eighteenth-century image of organisms as automata, machines or "living clocks".[40]

It is worth remembering that other contemporary readers besides Marx identified a technological aspect in Darwin's view of nature. Samuel Butler had sketched a curious portrait of the naturalist in a little-known article entitled *Darwin among the machines*, published in a New Zealand newspaper in 1863.[41] Butler too remarked on the hints at a history of human technology available in Darwin's work. Assuming the semiserious tone he made use of again in the "Book of the machines" of *Erewhon*,[42] Butler invited experts "both of natural history and of machinery" to undertake "the gigantic task of classifying machines into the genera and sub-genera, species, varieties and sub-varieties," as well as "of pointing out how subservience to the use of man has played that part among machines which natural selection has performed in the animal and vegetable kingdom".[43]

As for his own views, Marx's remarks on Darwin and the technology of nature reveal some additional facets of his interest in natural history. The idea that plants' and animals' organs are the "material basis" of animal life – as tools and machines are of human life – clearly appealed to Marx's philosophical materialism. The economist's interest in technology and the naturalist's interest in the organs responsible for animal life, pertained to a common ground, and could sustain each other within a materialistic world view. Nevertheless, at a different level, Marx wanted to underline the substantial *discontinuity* between the "technology" of the animal world and human technology. This emerges from Marx's reflections on the special forms of "labour" observable in the animal kingdom:

A spider conducts operations which resemble those of the weaver, and a bee would put many a human architect to shame by the constructions of its honeycomb cells. But what distinguishes the worst architect from the best of bees is that the architect builds the cell in his mind before he contructs it in wax.[44]

He further concluded: "the use and construction of instruments of labour, although present in germ among certain species of animals, is characteristic of the specifically human labour process, and Franklin therefore defines man as a 'tool-making animal'".[45]

Thus, in Marx's view, just as no single population law valid for both humans and animals can be conceived, so a continuous history of technology from animals to man would be objectionable. On what grounds, then, did Marx issue an invitation to develop a history of human technology inspired by the history of natural technology which Darwin had initiated? It seems reasonable to conclude that the invitation had roots in different, weaker assumptions. One such assumption was certainly Marx's conviction that both histories had to deal with the "material basis" of life, in nature as in society; and that, in this respect, man and nature were bound to similar procedures. As he noted again in *Capital*: "when man engages in production, he can only proceed as nature does herself, i.e. he can only change the form of the materials".[46] A second assumption of Marx's proposal – consequent though not explicitly stated – was probably that, given the common basis just mentioned, the adoption of similar cognitive strategies in the study of the two forms of technology should prove useful.

6. MARX'S IDEOLOGICAL USE OF DARWIN

The convergences and divergences so far detected in Marx's reflections on Darwin are, in our opinion, significant and instructive for the historian of Darwin's theory and its reception. They fall considerably short, however, of substantiating Marx's earlier and stronger claim that the however, of substantiating Marx's earlier and stronger claim that the *Origin* might offer "the basis in natural history" for his social and economic views. The question we are concerned with in this section is *how* such a claim could arise.

One answer is probably to be found in the confusion mentioned earlier in which Marx and Engels, like many other contemporary observers, blurred the distinction between the merits of Darwin's theory and those of the general idea of evolution. For example, Marx was clearly hinting at evolutionism, rather that at Darwin in particular, when he declared that in *Capital* "the development of the economic formation of society is viewed as a process of natural history," and that his work aimed at revealing "the economic law of motion of modern society".[47] The same was true of Engels when he credited the *Origin* with having demonstrated the "historical development of nature".[48] What both Marx and Engels saw here as a possible basis for their views in natural history was evolutionism rather than Darwin's theory itself. There is evidence, however, that in Marx's and Engels' search in the natural sciences for a basis for their social theories something else was at stake besides their genuine interest in biological evolutionism, and in an overall materialistic world view.

It has been convincingly demonstrated[49] that there was also an ideological side to Marx's use of Darwin, that Marx tried to exploit Darwin's prestige to promote *Capital*. The most straightforward testimony of this are two letters exchanged by Marx and Engels in December 1867.[50] A prominent issue they debated there was the easiest way to recommend the newly published first book of *Capital* to a German audience – the readers of *Der Beobachter* edited by Karl Mayer – that was particularly responsive to the appeal of "vulgar" materialism *à la* Karl Vogt of the kind severely criticized elsewhere by Marx. It was Marx who suggested the appropriate tactics to gain the favour of that audience, while Engels acknowledged that Marx's "recipe" was "very fine" [*sehr hübsch*], and quickly put it into practice by writing a review of *Capital* in line with Marx's recommendations.[51]

Suggesting the actual words that Engels should use, Marx invited him to emphasize the "positive" and "solid" achievements which *Capital* had attained by adopting the method of "materialism (a word that pleases Mayer because of Vogt)".[52] He suggested that Engels stress *Capital*'s demonstration that "cooperation, division of labour, the use of machines, and the connected social relations develop 'according to natural laws.'" Marx also told Engels to write that

> When he [Marx] shows that, from an economic viewpoint, present society is pregnant with a new and higher form, he is just showing from a social point of view the same process of transformation [Unwälzung] established by Darwin in natural history. The liberal theory of "progress" (*c'est Mayer tout pur*) concurs on this and it is the author's merit to reveal a hidden progress precisely where modern economic relations display discouraging immediate consequences.[53]

It is not surprising, given the context, that Darwin's name was being evoked here with reference to the most general issues of transformation and progress, rather than to those more specific and subtle aspects of his theory which Marx elsewhere showed he was able to appreciate.

The episode seems to confirm that Marx's interest in Darwin oscillated between two poles. On the one hand, there was a perceptive grasp of some of Darwin's original concepts concerning "the technology of nature," as Marx put it. On the other, there was the temptation, rather common among nineteenth-century social theorists, to legitimize the social with recourse to the natural sciences. No doubt, this second side of the Darwin-Marx relations came first in the battle of ideas being waged in the second half of the nineteenth century among the many European intellectuals and politicians aware of the appeal of evolutionism and/or socialism.[54]

7. CONCLUSIONS

Some concluding remarks seem appropriate to sum up the multi-faceted reality revealed by our examination of Marx's thoughts on Darwin; especially with a view at exposing how Marx's ideological concern in presenting his ideas on a par with biological theories, and thus as scientific, could merge with Marx's own perceptive interpretation of Darwin's theory. The controversial issue of the parallelism between Darwin's and Marx's views will offer the connecting thread.

As we have seen, Marx and Engels themselves conceived of this parallelism as holding primarily between their general, dynamic view of human history, and the evolutionary view in biology. At this level, in fact, a vague parallelism could be asserted with reference to *almost all* of the many theories of biological evolution being put forward during the central decades of the nineteenth century. Marx's sympathies for Trémaux indeed bear this out overwhelmingly.

At a somewhat different level, a parallelism was perceived by Marx with reference to Darwin's interest in the material basis of animal life (means of subsistence, organs and functions), an interest which in some ways corresponded to Marx's own interest in the material basis of human societies (resources, machines, modes of production). At this level – which was connected with the issue of philosophical materialism – a certain parallelism could be asserted with reference to *many* of the theories of biological evolution available at the time.

Above all, Marx and Engels conceived of the parallelism – of which they themselves saw the limited applicability and the many exceptions – as a mean for promoting their own view of society, presenting it as scientific and in keeping with the naturalism and materialism spread in the radical circles of their time. We saw this examining the letters Marx and Engels exchanged in 1867, leading to Engels' review of *Capital* in *Der Beobachter*. The episode showed that, while the proclaimed parallelism between Darwin's and Marx's views was to a large extent unsubstantiated, Marx and Engels were interested in using it as an ideological weapon in advertising their social views. Clearly, this ideological dimension was part of the complex interaction between the natural and the social sciences we have dealt with.

Yet, whatever the ideological or philosophical motives pushing Marx towards Darwin, Marx was also able to grasp some particular and little noticed traits of Darwin's theory, that he captured with his penetrating image of Darwin's theory as a theory about the history of natural technology. This interpretation of Darwin's theory on the part of Marx, though sketchy, is itself an interesting case in the history of the interaction between the natural and the social sciences.

Marx's interpretation confirms that the presence in Darwin's work of concepts and images inspired by economics was perceived by his contemporaries. It shows, furthermore, that the interaction perceived between the natural and the social sciences extended beyond Darwin's explicit borrowing from Malthusian concepts: it included Darwin's "technological" view of nature which at least one other contemporary, Samuel Butler, clearly noted. Besides, when Marx recommended that the history of human technology should be studied along lines similar to those drawn by Darwin for the study of natural technology, he displayed an inclination to favor conceptual borrowing between the natural and the social sciences. In fact, he suggested that historians should in this field adopt cognitive strategies inspired on Darwin's view of the organic world.

Evidently, in the central decades of the nineteenth century the boundaries between the natural and the social sciences could be bridged quite easily and effectively in the generation of new ideas. The well-known cases of Darwin, Alfred Russel Wallace,[55] and Henry Milne-Edwards[56] indicate that, in the first half of the century, conceptual borrowings often went *from* the social *to* the natural science. In the 1860s and 1870s, however, as a consequence of the extraordinary prestige acquired by the natural sciences, and by Darwin in particular, borrowings more often went in the *opposite* direction. Under such circumstances it might be tempting – though clearly unwarranted – to represent an interesting though limited case of interaction between the natural and the social sciences as a comprehensive parallelism between two theories from the two distinct fields. This is precisely what Engels did at Marx's graveside, carrying out a scheme Marx himself had suggested some fifteen years before under the pressure of Darwin's enormous prestige. In view of the astonishing variety of misrepresentations and misappropriations of Darwin's ideas current up to the turn of the century and beyond, however, the historian of Darwinism should refrain from condemning Marx and Engels too severely under this charge. Unless, of course, the historian himself is tempted to yield to the pressure of his own times.

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NOTES

[1] Karl Marx and F. Engels: *Werke* [MEW] 39 vols. (Berlin: Dietz, 1957–68), **30**: p. 578.

[2] MEW, 19: pp. 333, 335.

[3] The best reconstruction of the story, and an accurate record of the role played by different historians in sorting out the myth, has been offered by Ralph Colp Jr. (Ralph Colp Jr.: "The myth of the Darwin-Marx letter", *History of Political Economy*, 1982, **14**: 461–482); see also Gerhard H. von Müller: "Darwin, Marx, Aveling – Briefe und Spekulationen. Eine bibliographische Betrachtung", *Dialektik*, 1983, **6**: 149–159.

[4] See Howard E. Gruber; "Darwin and Das Kapital", *Isis*, 1961, **52**: 582–583; Julian Huxley, & H.B.D. Kettlewell: *Charles Darwin and His World* (London: Thames and Hudson, 1974), p. 80.

[5] Colp Jr.: (n. 3 supra), p. 463.

[6] Alfred Kelly's work on Germany is a notable exception: Alfred Kelly: *The Descent of Darwin. The Popularization of Darwinism in Germany, 1860–1914* (Chapel Hill: The University of North Carolina Press, 1981), Chap. 7; see also Linda L. Clark: *Social Darwinism in France* (Alabama: The University of Alabama Press, 1984), Chap. 5.

[7] Shlomo Avineri: "From hoax to dogma: a footnote on Marx and Darwin", *Encounter*,

1967, **28**: 30–32; Maurice Mandelbaum: *History, Man, and Reason. A Study of Nineteenth-Century Thought* (Baltimore and London: The Johns Hopkins University Press, 1971); Maurice Mandelbaum: *Philosophy, History, and the Sciences* (Baltimore and London: The Johns Hopkins University Press, 1984); G.A. Cohen: *Karl Marx's Theory of History. A Defence* (Oxford: Clarendon Press, 1978); Diane B. Paul: "Marxism, Darwinism, and the Theory of two sciences", *Marxist Perspective*, **2**: 116–143; Yves Christen: *Le grand affrontement. Marx et Darwin* (Paris: Albin Michel, 1981); Dominique Lecourt: "Marx au crible de Darwin", pp. 227–249 of Yvette Conry, (ed.): *De Darwin au Darwinisme: science et idéologie* (Paris: J. Vrin, 1983); *Raison présente*, monographic issue: "Darwin Marx", n. 66, 1983; Arthur L. Caplan & Bruce Jennings (eds.): *Darwin, Marx, and Freud. Their Influence on Moral Theory* (New York and London: Plenum Press, 1984).

[8] Maurice Mandelbaum: *History, Man, and Reason* (n. 7 supra); Giuliano Pancaldi: *Charles Darwin: "storia" ed "economia" della natura* (Firenze: La Nuova Italia, 1977), Part II, Chapt. 1; I. Bernard Cohen: *Revolution in science* (Cambridge Mass. and London: The Belknap Press of Harvard University Press, 1985), Chapt. 23.

[9] (Hamburg: Meissner, 1872).

[10] MEW, 29: p. 524.

[11] On Darwin and teleology see Dov Ospovat: *The Development of Darwin's Theory* (Cambridge: Cambridge University Press, 1981), *passim*, and Ernst Mayr: "The concept of finality in Darwin and after Darwin", *Scientia*, 1983, **77**: 97–117.

[12] See for example Mario A. Di Gregorio: *T.H. Huxley's Place in Natural Science* (New Haven and London: Yale University Press, 1984).

[13] MEW, 30: p. 131.

[14] MEW, 30: p. 578.

[15] See Robert M. Young: *Darwin's Metaphor. Nature's place in Victorian Culture* (Cambridge: Cambridge University Press, 1985); David Kohn: "Theories to work by: rejected theories, reproduction, and Darwin's path to natural selection", *Studies in History of Biology*, 1980, **4**: 67–170; Silvan S. Schweber: "Darwin and the political economists: divergence of character", *Journal of the History of Biology*, 1980, **13**: 195–289; Silvan S. Schweber: "The wider British context in Darwin's theorizing," pp. 35–69 of David Kohn (ed.): *The Darwinian Heritage* (Princeton: Princeton Unviersity).

[16] NEW, 30: p. 249.

[17] See Camille Limoges: *La sélection naturelle* (Paris: Presses Universitaires de France, 1970); and Giuliano Pancaldi (n. 8 supra).

[18] Pierre Trémaux: *Origine et transformation de l'homme et des autres êtres. Première partie indiquant la transformation des êtres organisés, la formation des espèces, les conditions qui produisent les types, l'instinct et les facultés intellectuelles, la base des sciences naturelles, historiques, politiques, etc.* (Paris: Hachette, 1865).

[19] NEW, 31: p. 248.

[20] On the reception of Darwinism in France see Yvette Conry, *L'introduction du darwinisme en France au XIXe siècle* (Paris: Vrin, 1974).

[21] Trémaux was not a member. In 1864, however, he submitted to the Académie a number of memoirs on his travels to Africa and on transformism, which received mixed reviews. See *Comptes rendus de l'Académie des sciences*, 1864, **58**: 352–3, 526–528, 610–612, 692, 752–755, 1097–8, and **59**: 33, 204, 1197.

[22] Trémaux, (n. 18 supra), p. 227 ff., esp. p. 232.

[23] Ibid., p. 435.
[24] Marx read Trémaux in summer 1866 (MEW, 31: pp. 247–9). However, there is no mention of Trémaux's book in Marx's published works, and apparently they did not exchange letters. Marx's interest in Darwin, on the other hand, is documented again in 1867 and in 1872-3 (see below).
[25] Henry Thomas Buckle: *History of civilization in England.* 2 vols. (London: Parker and Son, 1857–61); John Stuart Mill: *A System of Logic.* Vol. VIII of J.S. Mill: *Collected Works*, ed. J.M. Robson (Toronto and London: University of Toronto Press: 1961–1974) p. 931; see Ludmilla J. Jordanova: "Earth science and environmental medicine: the synthesis of the late Enlightenment" pp. 119–146 of L.J. Jordanova and Roy S. Porter, eds.: *Images of the Earth. Essays in the History of the Environmental Sciences* (Chalfont St Giles: The British Society for the History of Science, 1979).
[26] MEW, 31: pp. 259–60.
[27] MEW, 31: p. 530.
[28] See n. 4, supra.
[29] Karl Marx: *Capital. A Critique of Political Economy*, vol. 1, transl. Ben Fowkes, intro. by Ernest Mandel (Harmondsworth: Penguin Books, 1976), p. 650.
[30] Ibid., p. 651.
[31] Ibid., p. 784.
[32] MEW, 30: p. 249.
[33] Marx, (n. 29 supra), p. 380.
[34] MEW, 30: p. 249.
[35] Marx, (n. 29 supra), p. 493 n.
[36] Ibid., p. 286.
[37] Charles Darwin: *On the Origin of Species.* Facsimile reprint, 1964, intro. by Ernst Mayr (Cambridge Mass.: Harvard University Press, 1859), p. 149; cfr. Marx, (n. 29 supra), p. 461 n.
[38] See however, on Darwin and breeding as a form of technology, John F. Cornell: "Analogy and technology in Darwin's vision of nature", *Journal of the History of Biology*, 1984, **17**: 303–344.
[39] Charles Darwin: *Charles Darwin's Natural Selection. Being the Second Part of His Big Species Book written from 1856 to 1858.* Ed. R.C. Stauffer (London and New York: Cambridge University Press, 1975), p. 208.
[40] See Schweber: "The wider British context in Darwin's theorizing" (n. 15 supra), esp. p. 45 f.; on the wedge metaphor and species competition see M.J.S. Hodge and David Kohn: "The immediate origins of natural selection" pp. 185–206 of Kohn (n. 15 supra), p. 194; for a psychoanalytic interpretation of the same metaphor: Ralph Jr. Colp: "Charles Darwin's vision of organic nature", *New York State Journal of Medicine*, September 1979: 1622–1629.
[41] Samuel Butler: *The Note-Books of Samuel Butler.* Ed. Henry Festing Jones (London: A.C. Fifield, 1913), p. 42–7.
[42] Samuel Butler: *Erewhon* (London: A.C. Fifield, 1911).
[43] Butler (n. 41 supra), p. 43.
[44] Marx (n. 29 supra), p. 284.
[45] Ibid., p. 286.
[46] Ibid., p. 133.

[47] Ibid., p. 92.
[48] MEW, 29: p. 524.
[49] Enrique M. Ureña: "Marx and Darwin", *History of Political Economy*, 1977, **9**: 548–559. esp. p. 557 ff.
[50] MEW, 31: pp. 403–5.
[51] MEW, 16: pp. 226–228.
[52] MEW, 31: p. 404.
[53] MEW, 31: p. 404.
[54] See James Allen Rogers: *Russia: Social Sciences*, pp. 256–268 of Thomas F. Glick (ed.), *The Comparative Reception of Darwinism* (Austin and London: University of Texas Press, 1974); Kelly (n. 6 supra), Chapt. 7; Clark (n. 6 supra), Chapt. 5; Giuliano Pancaldi, *Darwin in Italy. Science across Cultural Frontiers* (Bloomington: Indiana University Press, 1991), Chapt. 6.
[55] H.L. McKinney: *Wallace and Natural Selection* (New Haven and London: Yale University Press, 1972), John Landgon Brooks: *Just before the Origin: Alfred Russel Wallace's theory of evolution* (New York: Columbia University Press, 1984).
[56] Schweber: "Darwin and the political economists: divergence of character" (n. 15, supra), p. 250 ff.

VICTOR L. HILTS

8. TOWARDS THE SOCIAL ORGANISM: HERBERT SPENCER AND WILLIAM B. CARPENTER ON THE ANALOGICAL METHOD

During the middle of the nineteenth century, the British philosopher Herbert Spencer made the organismic analogy central to evolutionary sociology.[1] Societies and organisms are analogous, Spencer wrote in a paper on the "social organism" which he published in 1860, because of the fact that they both "slowly augment in mass; that they progress in complexity of structure; that at the same time their parts become more mutually dependent."[2] For Herbert Spencer, the organismic analogy suggested a naturalistic basis to society; and it implied a continuity between life in its simplest and in its most complex forms. "Through all its ramifications," Spencer wrote, "Society is a growth and not a manufacture."[3] Spencer's analogy of the social organism is significant not merely because of its great importance for the history of social thought, but also because it illuminates an important aspect of scientific argument during the mid nineteenth century. Whereas many British philosophers had maintained that valid science should be grounded in experiment and induction, Herbert Spencer attempted instead to found his science upon the principle of analogy.

At first glance, Spencer's reliance upon analogy may seem highly ironic. How, one might ask, was it possible for the philosopher who, above all else, sought a naturalistic understanding of society to turn his back upon the experimental method? To raise this question is to suggest a possible contradiction in Spencer's thought. It might be suggested that Spencer was an inherently unscientific thinker whose ideas concerning scientific methodology bore little relationship to those of his more scientifically-qualified contemporaries. Indeed, Spencer's preference for argument by analogy may seem idiosyncratic, an assessment which Spencer himself did little to deny. Perhaps then, one must seek the basis of Spencer's faith in the power of analogy not in science but in philosophy, or even political ideology. It has been suggested by J.D.Y. Peel, for example, that Spencer's analogy of the social organism is best understood as a reaction on Spencer's part against excessively mechanistic models of society, and thus that it owed its origins more to political

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 275–303.

and social considerations than to science.[4] Peel has also characterized Herbert Spencer as the "last of the *Natürphilosophen*."[5] Spencer himself noted in his *Autobiography* that his reading during 1849 or 1850 of Samuel Taylor Coleridge's posthumous *Idea of Life*, inspired by Coleridge's admiration for *Natürphilosophie*, made a lasting impression.[6] More recently, Robert J. Richards has suggested that Spencer's evolutionary theory in general "was formed to meet the demands of his ethics."[7]

Although the motivation for much of Herbert Spencer's evolutionary speculations undoubtedly lies in Spencer's political and moral concerns, these concerns cannot similarly explain the particular character of Spencer's science. Nor, for this very reason, can they totally explain Spencer's attempt to place social thought upon a naturalistic footing. The relationships between Spencer's views on politics, ethics, and science were many and complex – and no attempt to explore these relationships in their generality will be attempted here. Far from being out-of-step with his scientific contemporaries, however, Herbert Spencer relied upon analogy in his own work in part because many other Victorian biologists, including those to whom Spencer was most deeply indebted for scientific information, also relied upon analogy. This is a point upon which Spencer himself insisted. In publishing his paper on the social organism, Spencer announced that his purpose was "to show what are the analogies which modern science discloses to us."[8]

Both J.D.Y. Peel and Robert J. Richards have noted that Herbert Spencer was greatly influenced by Karl Ernst von Baer's laws of embryological development and by Henri Milne-Edwards' concept of the physiological division of labor.[9] It is also well-known that Spencer learned of von Baer's embryological laws by his reading of a physiological textbook written by the eminent English physiologist, William B. Carpenter. Carpenter, however, was much more than merely the intermediary through whom Spencer learned about von Baer. Particularly important for an understanding of Herbert Spencer's views regarding the nature of science is the fact that Herbert Spencer encountered in Carpenter's physiological textbooks an explicit defense of analogy as a means of arriving at scientific generalizations. In this paper I will examine Carpenter's statements about the role of analogy in science, and discuss their possible influence upon Herbert Spencer. This examination will show that Spencer's contention that society is a "social organism" was, within the context of Spencer's own scientific readings, by no means a methodological aberration.

Before examining the connection between Herbert Spencer's ideas regarding the method of analogy and the views of William B. Carpenter, it is appropriate to make some preliminary remarks regarding views of scientific methodology in the early nineteenth century, especially as those views affected ideas regarding social science. The early nineteenth century was a period when many natural philosophers had become conscious of the distinctiveness of the scientific enterprise. In their efforts to understand and to justify science, they frequently appealed to what might, for sake of brevity, be called three canons regarding nature: the *unity of nature*, the *uniformity of nature*, and the *universality and invariability of natural law*. Together the three canons promised that scientific generalizations, once established, would remain valid for all time and in all places. Equally important for the development of social thought, these three canons promised that the scientific method could be extended into new areas, other than those in which it had received its first triumphs.

Because early nineteenth-century natural philosophers firmly believed in the above three canons, they often sought to establish relationships between the different branches of science. For this reason, the history of the social sciences in the early nineteenth century must be understood very differently than the history of social science in the latter half of the century. A characteristic feature of the human sciences in the late nineteenth and early twentieth centuries was the emergence of disciplinary specialties. The establishment of new disciplines was usually accompanied by a search for distinctive methods and concepts; these, in turn, helped to establish what historians have frequently termed "disciplinary boundaries". By the end of the nineteenth century, the development of new social science disciplines meant the gradual divergence of important aspects of social science away from models based directly upon the natural sciences, as social scientists strove to articulate concepts and invent methods appropriate to their own subject matter. By contrast, would-be social scientists of the earlier part of the nineteenth century were often insistent that their endeavors were fundamentally the same as those of the natural sciences. And instead of seeking to create distinctive scientific disciplines, they frequently attempted to create a general social science.

In their attempts to establish relationships between the moral and social sciences on the one hand and the natural sciences on the other hand, many thinkers during the early nineteenth century were nonetheless willing to admit that the two areas of science differed in one crucial respect: the opportunity for experiment. The test case for much early nineteenth-century discussion about the methodological foundations of social science was political economy. But could political economy be made experimental? The noted political economist John Ramsay McCulloch proclaimed in his article on political economy for the *Encyclopaedia Britannica* that "political economy is not a science of speculations, but of fact and experiment."[10] Few political scientists, however, were so certain that the experimental method could be applied to economic phenomena. The necessity of erecting foundations for a social science based upon induction that could not rely upon experiment was a problem that came to the fore in Mill's *System of Logic*, published in 1843. Inductivist though he was, John Stuart Mill admitted that the opportunity for experiment in social and moral science was much less than in the physical sciences.[11] Mill observed, for example, that one could not discover the effect of free trade upon national prosperity by experiment since no two countries were identical in all respects except tariff policy.[12] In the end Mill's *System of Logic* yielded not a defense of existing social and moral sciences but rather a prescription for new sciences of man. Mill suggested that the moral and social sciences of the future would in some cases resemble a hybrid science like astronomy, where the opportunity for experiment was also limited.

The question that Mill addressed – how a moral science could be scientific if it was not experimental was determined for many thinkers by their faith in the unity of nature and the universality of natural law. Even where experiments could not be made, natural laws, they believed, still existed. This point of view was advanced by both the French founder of positivism and prophet of sociology, Auguste Comte, and by John Stuart Mill himself. Auguste Comte wrote in his *Cours de philosophie positive* that "The first characteristic of the Positive Philosophy is that it regards all phenomena as subjected to invariable natural *Laws*."[13] In the preface to his *System of Logic*, John Stuart Mill stated that he hoped solve the question, "Whether moral and social phenomena are really exceptions to the general certainty and uniformity of the course of nature; and how far the methods, by which so many of the laws of the physical world have been numbered among the truths irrevocably acquired and universally assented to, can be instrumental to the formation of a similar body of received doctrine in moral and political science."[14]

At one level the phrase "laws of nature" suggested the achievements of a Galileo or a Newton, but at another level – historically and psychologically more profound – the same phrase pointed to a natural Order (perhaps Divinely created) in terms of which the actions of mankind possess moral significance. Even those who did not share Comte's conviction that the positivistic stage of human thought was destined to supersede the theological stage could believe that in some fashion mankind was ruled by the "laws" of morals in much the same way that physical objects were ruled by the "laws" of physics. In his *Elements of Moral Science*, Francis Wayland of Brown University in the United States, for example, wrote that moral law, like physical law, is a "form of expression denoting either a mode of existence or an order of sequence." Wayland believed an order of sequence implied an "Establisher" and for this reason Wayland argued, "an order of sequence once discovered in morals, is just as invariable as an order of sequence in physics," Wayland argued that the only difference between the laws of physics and those of morals was the time that elapsed between cause and effect – in physics "the consequent follows the antecedent often immediately" whereas in morals "the result is frequently long delayed."[15]

A coalescence of the scientific and normative meanings of the phrase "laws of nature" occurred in the economic writings of the eighteenth-century French physiocrats. According to the physiocrats, James Bonar observed, "if men violate physical laws, they will suffer death; and if they violate the laws of social order, which are equally natural, they will ruin and destroy each other."[16] Physiocratic ideas, in turn, greatly influenced classical political economy and through political economy the evolutionary social science of the mid nineteenth century. The concept of unvarying laws of nature was also central to the social thought of the phrenologists. Best-known for their views regarding cerebral localization, the phrenologists were also in some ways the best representatives of a link between the natural and the social sciences during the early nineteenth century. Phrenologists argued that man must "obey" the laws of nature since contravention, it was claimed, would inevitably lead to failure and unhappiness. In 1828 the phrenologist George Combe wrote in his *Constitution of Man Considered in Relation to External Objects*: "Physical laws of nature, affecting our physical condition, as well as regulating the whole material system of the universe, are universally acknowledged, and constitute the elements of natural philosophy and chemical science. Physiologists, medical practitioners, and all who take medical aid, admit the existence of *organic laws*; and the science of government, legislation, education, indeed our whole train of conduct through life, proceed, upon the admission of laws of morals."[17] This notion of "obedience" to natural law enabled the phrenologists to represent phrenology as at once a natural and a social science. Phrenologists urged their audience towards self-improvement; but self-improvement, they always insisted, must be guided by an understanding of natural law and man's relationship to that law.[18]

Whereas phrenologists merely asserted the existence of universal laws affecting both man and nature, some other social thinkers attempted to provide a philosophical foundation for the discovery of such laws. Henry Thomas Buckle, for example, hoped to turn history into a social science by the publication in 1857 of his *History of Civilization in England.* As did John Stuart Mill with the moral sciences more generally, Buckle recognized that there were many impediments to the creation of a science of history, including the complexity of its subject matter and the lack of opportunity for experiment. "As to the greater complexity of the phenomena," wrote Buckle, "the philosophic historian is opposed by difficulties far more formidable than is the student of nature; since, while on the one hand his observations are more liable to those causes of error which arise from prejudice and passion, he, on the other hand, is unable to apply the great physical resource of experiment, by which we can often simplify even the most intricate problems in the external world."[19] Buckle's alternative to experiment lay in the formulation of general historical laws in statistical terms.

Herbert Spencer's view of science must also be placed within the context of early nineteenth century belief in the unity of nature, the uniformity of nature, and the universality and invariability of natural law. For Spencer – as for Auguste Comte, John Stuart Mill, the phrenologists, and Thomas Henry Buckle – belief in the canons of early nineteenth century science suggested that the scientific method could be extended to an understanding of mankind himself. Unlike Buckle, Spencer did not attempt to bolster his belief in these canons of early nineteenth century science by an appeal to statistics; and, unlike Mill, Spencer did not seem to worry about the inapplicability of the experimental method to the study of mankind. Spencer's belief in the canons of nineteenth century science was instead mediated and strengthened by his understanding of what he considered to be the implications of mid nineteenth century biology. Herbert Spencer was convinced that biologists of the early nineteenth century had already arrived at several great scientific generalizations, and that the time had come to extend these generalizations to man and society. The biological generalizations that Spencer had in mind did not, of course, include Charles Darwin's theory of natural selection, which was published only after Herbert Spencer's own evolutionary ideas had been formulated. Much of Spencer's early biological knowledge was derived from his reading of William B. Carpenter's *Principles of General and Comparative Physiology*, the first edition of which had been published approximately a decade before Spencer began to think about biological topics. In this work, Carpenter encountered both a discussion of Karl Ernst von Baer's embryological laws and a strong defense of the analogical method. Carpenter's own views regarding analogy, however, must be placed within two settings: the one methodological and the other biological.

WILLIAM B. CARPENTER

The propriety of analogical reasoning in philosophical discourse was debated by those interested in scientific methodology long before Herbert Spencer became convinced that society was organism. Scottish philosophers of the common sense school at the end of the eighteenth century, for example, were particularly interested in the use and misuse of analogy within inductive natural philosophy. Richard Olson has shown that Scottish methodological discussions had a direct, if belated, influence on several mid-Victorian scientists, even providing a methodological background for James Clerk Maxwell's search for mechanical analogies for electricity and magnetism.[20] Indicative of eighteenth-century Scottish interest in the methodological foundations of analogy was the article on "analogy" in the first edition of the *Encyclopaedia Britannica*, published in Edinburgh. In general, Scottish philosophers were wary of too great an appeal to analogy, but the first edition of the *Encyclopaedia Britannica* affirmed the utility of analogical reasoning and stated that the use of analogies was justified by the existence of general laws in nature: "A great part of our philosophy has no other foundation than analogy, the utility of which consists in superseding all necessity of examining minutely every particular body; for it suffices us to know that every thing is governed by general and immutable laws, in order to regulate our conduct with respect to all similar bodies, as we may reasonably believe that they are all endowed with the same properties: Thus, we never doubt that the fruit of the same tree has the same taste."[21]

One of the most influential early nineteenth-century discussion of analogy was contained in John Herschel's *Preliminary Discourse on the Study of Natural Philosophy*, published in 1830 in the *Library of Useful Knowledge*. Herschel argued that it was important for natural philosophers to extend their generalizations as widely as possible so as to discover the universal laws of nature, and he maintained that recognition of analogies was a necessary step in this process of generalization.[22] For John Herschel, as for the Scottish philosophers, the foremost example of a successful scientific generalization was the theory of universal gravitation put forth by Isaac Newton in his *Philosophiae Naturalis Principia Mathematica*. Not coincidentally, Newton's second rule of reasoning in philosophy as stated in the *Principia* ("to the same natural effects we must, as far as possible, assign the same causes") was sometimes interpreted as a defense of analogical reasoning.[23] This interpretation of Newton's second rule was not limited to the methodological writings of philosophers of science, but also appeared in more popular literature. The article on "analogy" published during the mid nineteenth century in Francis Lieber's *Encyclopaedia Americana*, for example, stated that "Newton gives analogy the second place amongst his laws of philosophizing, and may be said to have established some of the most characteristic parts of his system, as arising out of the doctrine of gravitation, on its sober and patient use. In fact, analogical reasoning is essential in inductive philosophy, though it must be used with caution."[24]

John Stuart Mill's *System of Logic* gave reserved sanction to the use of analogy, but Mill's *System of Logic* was more critical of argument by analogy than was John Herschel's *Preliminary Discourse on the Study of Natural Philosophy*. Mill defined one meaning of analogy as "some kind of an argument supposed to be of an inductive nature, but not amounting to a complete induction." Mill allowed that in some cases analogies could increase the probability of an hypothesis.[25] But Mill emphasized that analogies so employed must be "true analogies". In a section of the *System of Logic* devoted to the "fallacies of generalization", Mill provided a lengthy and somewhat sarcastic discussion of what he called "false analogies":

[An error or fallacy of analogy] . . . is sometimes supposed to be particularly incident to persons distinguished for their imagination; but in reality it is the characteristic intellectual vice of those whose imaginations are barren, either from want of exercise, natural defect, or the narrowness of their range of ideas. To such minds objects present themselves clothed in but few properties; and as, therefore, few analogies between one object and another occur to them, they almost invariably overrate the degree of importance of those few; while one whose fancy takes a wider range perceives and remembers so many analogies tending to conflicting conclusions, that he is much less likely to lay undue stress upon any of them. We always find that those are the greatest slaves to metaphorical language who have but one set of metaphors.[26]

The methodological writings of the Scottish philosophers, as well as those of John Herschel and John Stuart Mill, usually borrowed their examples from astronomy and physics. As the nineteenth century progressed, however, a different set of sciences – and therefore a different image of the scientific enterprise itself – was to emerge. The chemical revolution and, especially, nineteenth-century biology provided scientists with a view of science based upon new and different principles. At least initially, however, biological social science built upon rather than overturned the canons of the unity of nature, universality of natural law, and uniformity of nature that guided early-nineteenth-century scientists. For many biologists of the early nineteenth century, the critical problem was to discover a unity amongst diversity. This task, in turn, led to the search for biological analogies, both between individual organisms and between the parts of individual organisms. "The discovery of analogies played a very important part," John Theodore Merz wrote in his *History of European Thought in the Nineteenth Century*, "in the study of these typical forms and structures in which nature repeats itself, reverting again and again to them, but in every single case departing more or less from them."[27] As Merz was aware, the search for analogies – or, at least, of "analogues" – engaged many biologists who were inspired by German *Natürphilosophie*, but the search was by no means limited to them alone.

During the early nineteenth century, the quest for analogies pervaded the work of several anatomists in France and Germany. The French comparative anatomist Geoffroy Saint-Hilaire, for example, became convinced as early as his study of the Madagascar apes, published in the *Magasin Encyclopédique* in 1796, that "nature has shut herself up within certain limits, and has formed all living beings on one sole plan, essentially the same in principle, but varied in a thousand ways in all the accessory parts."[28] In 1818 Saint-Hilaire published the first volume of his *Philosophie anatomique: – Des organes respiratoires sous le rapport de la détermination et de l'identité de leur pieces osseuses*, in which he developed the idea that the "unity of composition" of the animal kingdom may be demonstrated by the anatomical analogies between different members of the kingdom. "That we shall always find in each family the organic parts which we have observed in another," Saint-Hilaire wrote, "are what I have embraced in this work under the denomination of the Theory of Analogues."[29] The claim provoked the celebrated scientific controversy that culminated in the debate between Geoffroy Saint-Hilaire and George Cuvier in the Académie des Sciences in Paris in 1830. Among those following this debate attentively was Richard Owen, the young English medical student. Fifteen years after the publication of Geoffroy Saint-Hilaire's *Philosophie anatomique* the problem of biological relationships and analogies led to Owen's distinction between analogous and homologous organs. Owen defined an "analogue" as "a part or organ in one animal which has the same function as another part or organ in a different animal" and a "homologue" as "the same organ in different animals under every variety of form and function."[30]

One illustrative, though specialized, attempt to discuss the methodological basis of analogical reasoning in biology was contained in the writings of the so-called quinarians. In the 1820s the entomologist Robert Macleay began to advocate what he called the "quinary" or circular system of biological classification. Macleay's *Horae Entomologicae*, published in two volumes in 1819–21, argued that "Relations of analogy consist in a correspondence between certain insulated parts of the organization of [two] animals which differ in their general structure."[31] In his *Philosophie zoologique*, J.B. Lamarck had used the terms "analogy" and affinity" nearly synonymously, writing that "among living bodies the name affinity has been given to features of analogy or resemblance between two objects, that are compared in their totality, but with special stress on the most essential parts."[32] Macleay, by contrast, distinguished between "analogy" and "affinity". Macleay's views were adopted by William Swainson, who in 1834 published a work in the *Library of Useful Knowledge* with a title parallel to that of Herschel's publication of four years earlier, *Preliminary Discourse on the Study of Natural History*. One chapter of Swainson's *Preliminary Discourse* was entitled, "On the Importance of Analogy when Applied to the Confirmation of Theory".[33] Swainson wrote that "analogies are, in the most comprehensive sense of the word, universal; . . . they consequently assume an importance of the highest order when applied to illustrate, and to confirm, any theory on the variation of animal structure."[34] The views on analogy held by the quinary theorists were sufficiently important to receive notice in 1837 in William Whewell's *History of the Inductive Sciences*. Whewell, however, curtly dismissed the claims of the quinary theorists for a "relation of *Analogy* different from Affinity" on the grounds that their claims were not consistent with "the gradual approximation of the classificatory to the physiological sciences."[35]

During the early nineteenth century, there were thus two separate contexts within which the interpretation of analogical reasoning had a place – the one methodological, and stemming from Scottish discussions of induction; and the other biological, and related in part though not exclusively to the influence of German *Natürphilosophie*. William B. Carpenter forged a link between these methodological and biological meanings of analogy. Phillip F. Rebock has included Carpenter among the British "philosophical naturalists" who were greatly influenced by German *Natürphilosophie*, and Carpenter was, indeed, sympathetic to the goals of *Natürphilosophie* even though he was equally influenced by the philosophical writings of the Scottish philosophers and by the inductive philosophy of science of John Herschel.[36] On April 14, 1837 Carpenter, just then completing his medical studies, read a paper to the Royal Medical Society of Edinburgh entitled, "On the Unity of Function in Organized Beings."[37] Carpenter began by noting that a generalizing spirit had recently been aroused among natural scientists and that formerly isolated anatomical and physiological facts could at last begin to be understood within the framework of broad, though still provisional, generalizations. Carpenter stated that he was in agreement with this new era in biological thought, and he defended the German *Natürphilosoph* Lorenz Oken, whose followers Carpenter thought had been unfairly ridiculed for arguing that wings could be considered "aerial gills". According to Carpenter there was nothing farfetched about Oken's claim. That the structure of a wing "is exactly analogous to that which exists in the gills of aquatic insects," he contended, "will, I think, ultimately appear to be supported by the strictest analogy in structure, situation, and development."[38]

In 1839 Carpenter published a volume whose title indicated that it had methodological as well as expository aims, *Principles of General and Comparative Physiology, intended as an Introduction to the Study of Human Physiology and as a Guide to the Philosophical Pursuit of Natural History*. Carpenter's volume was destined to go through several editions and to become one of the principal physiological textbooks of its time. Carpenter argued that establishing laws of great generality was the ultimate goal of science. Repeating Cuvier's rhetorical "Why should not Natural History some day have its Newton?", he replied: "Although the labours of the Natural and Comparative Anatomist have not yet established laws of the *highest* degree of generality – the discovery of which *may* perhaps be reserved for another Newton – many subordinate laws have been based on a solid foundation and many more, which were at first doubtful, are daily receiving fresh confirmation."[39] Carpenter did more than simply state his expectations; he also described what he believed was involved in the process of scientific generalization. "In comparing phenomena of any kind for the purpose of arriving at a law common to them all," he wrote, "it is necessary to feel certain that they are of a *similar character*."[40] Creation of scientific laws, in other words, depended upon the recognition of analogies. "The brilliancy of Newton's genius," Carpenter opined, "was shown in the perception that the fall of a stone to the earth, and the motion of the moon around it, were *analogous* phenomena, subject to the same law; not in the mere deduction of a numerical law from the ratios supplied by those facts."[41] Carpenter dedicated his *General and Comparative Physiology* to John Herschel, "as a tribute due alike to his high scientific attainments, and moral worth, and as an expression of gratitude for the benefit derived from his 'Discourse on the Study of Natural Philosophy' by the author."[42] In his text Carpenter noted Herschel's belief in the uniformity of nature and the presence of underlying common causes. Citing Herschel's *Preliminary Discourse on the Study of Natural Philosophy*, Carpenter wrote:

> Our belief in the uniformity of Nature, which leads us to seek for a common cause when a number of similar phenomena are presented to our observation, is based, not only upon experience, but upon the conviction which every believer in the existence of the Deity feels of his immutability. If it were otherwise, we should be led by *analogy* only to infer the existence of law and order when none is evident.[43]

Carpenter was not himself Scottish, but he took his medical degree from Edinburgh, where he undoubtedly came into contact with the Scottish discussions of inductive scientific methodology. As suggested by the above passage, however, another factor also entered into Carpenter's views regarding the nature of science. An active unitarian, William B. Carpenter was greatly interested in the bearings of science upon religion; indeed, many of his views regarding scientific methodology reflected his desire to demonstrate a harmony between science and religion. It was probably because of his latter interest that Carpenter became aware of the views regarding analogy that were advanced during the 1830s by the Oxford professor of geometry, Baden Powell.[44] Early in his scientific career, Baden Powell was involved in research on latent heat, but later he became interested in scientific exposition, the history and philosophy of science, and in the relationship between science and religion. Baden Powell had given great thought to the nature and consequences of analogical reasoning, first as it involved questions of physics and then as it involved matters of theology.

In the mid 1830s Baden Powell participated in the debates concerning the possibility of providing a more rigorous formulation of Newton's three laws of motion than that given in the *Principia*, and on November 30, 1837 Baden Powell read to the Ashmolean Society a lecture entitled "On the Nature and Evidence of the Primary Laws of Motion."[45] Baden Powell suggested that Newton's law of action and reaction had confused different "sorts of action," impact and gravitation, that – he believed – were not analogous:

> In short it appears to me that . . . there is a very large infusion of verbal mysticism: certain specious and plausible expressions are employed to comprehend a most vague and uncertain meaning; to class together objects which have nothing in common; some of which are correct and important facts, and others merely unmeaning abstractions, adopted solely for the purpose of producing an apparent and nominal generalization of particular effects, having no real analogy or connexion.[46]

Carpenter probably did not learn of Baden Powell's views on analogy from Powell's Ashmolean lecture (Carpenter was in Edinburgh when it was delivered) but rather from Powell's views on the relationship of science and religion. Like Carpenter himself, Baden Powell believed that science and religion were not incompatible. These views were expressed by Baden Powell in 1838 in an essay entitled *Connexion of Natural and Divine Truth*, an essay known to William B. Carpenter when Carpenter was writing *General and Comparative Physiology*. In his *General and Comparative Physiology*, Carpenter noted that his use of "analogy" was in keeping with Baden Powell's terminology; and from Baden Powell's *Connexion*, Carpenter quoted (and Herbert Spencer undoubtedly later read):

> The most important part of the process of induction consists in seizing upon the probable connecting relation, by which we can extend what we observe in a few cases to all. In

proportion to the justness of this assumption, and the correctness of our judgement in tracing and adopting it, will the induction be successful. The analogies to be pursued must be those suggested from already-ascertained laws and relations.[47]

Carpenter did not quote in his *General and Comparative Physiology*, but undoubtedly he nevertheless read, a yet stronger statement regarding the importance of analogies that Baden Powell included in the chapter on induction in the latter's *Connexion of Natural and Divine Truth*:

In replying then, to the inquiry, What constitutes the ground of antecedent probability, so essential to a good induction? it will be almost apparent, from the examples already cited, that the main ground is that afforded by the *comparison of one class of phenomena with another*: the perception of a *parallelism* in their respective conditions: the existence of an ANALOGY between them.[48]

For William B. Carpenter, the employment of analogy was a two-edged sword. The establishment of analogies could contribute to the formation of wide-sweeping scientific generalizations; but, on the other hand, false generalizations could be shown to be fallacious by a critical examination of the analogies on which they were based. Carpenter, himself, discussed analogies both when he wanted to establish a scientific generalization and when he wanted to show the error of a scientific generalization. Carpenter, for example, applied the analogical test to claims that vital phenomena could be reduced to, or were no more than, inorganic materials. Carpenter rejected these claims as wanting in analogy:

However close . . . may be the links by which the animal and vegetable kingdoms are connected together, the relation is only a mutual one; and between organised fabrics, and the products of crystallisation, (or of any other mode of aggregation by which inorganic matter is held together, in masses great or small,) there is a total want of resemblance. In this instance no analogy can be traced, except what is vague and chimerical.[49]

Carpenter likewise rejected vitalistic claims for the existence of a special life force incapable of description in terms of natural laws; instead he sought a ground intermediate between vitalism and mechanism by arguing that scientific laws would eventually be discovered in all phenomena.

One of the Carpenter's central ideas was that biological analogies must be based upon a knowledge of biological development. Carpenter was one of the first British writers to appreciate the importance of Karl Ernst von Baer's laws of embryological development, and, according to Dov Ospovat, the various editions of Carpenter's *General and*

Comparative Physiology "did more than any other work to make von Baer's ideas known to the English-speaking world."[50] Familiarity with von Baer's work led Carpenter to see the unity of nature in developmental terms, and thus to depart from the more static viewpoint of pure comparative anatomy. Although indicating his support for Oken's attempt to establish analogies in his 1837 paper on the unity of function in organized beings, for example, Carpenter noted in that paper that the test of analogy could no longer be limited simply to "similarity in function and external form." "The time has long gone by," he wrote, "when similarity in function and external form were considered sufficient for recognition of analogies between organs; anatomists are now aware of necessity of resting their comparison upon the elementary structure of their organs, their connections with each other, and the changes that they undergo during the process of their development."[51]

Illustrating William B. Carpenter's developmental approach to the study of biological analogies was an article that Carpenter and the American scientist James D. Dana published jointly in 1851. Its title was, "On the Analogy between the mode of Reproduction in Plants and the 'Alternation of Generations' observed in some Radiata". The article was inspired by observations that a polyp and a Medusa could be different generations of the same species; the eggs of the Medusa becoming polyps; and the polyps producing a bud that yielded a Medusa. Carpenter and Dana argued that the alternation was not unusual, and that it was in fact analogous to what occurred in plants where, they claimed, a "seed produces leaf individuals; and these produce seeds; precisely, as the egg produces polyps, and the polyps bulbs, that develop into Medusae, and the Medusae eggs." For Carpenter and Dana, the analogy was a real one that indicated the existence of a general law of growth and development. "We find the analogy completely sustained even in minor points of structure and growth," they wrote, adding that "the only point in which the analogy seems to fail, is that the Medusa bud falls off before its full development, while this is not so with the plants." The concluding paragraph was: "The law of alternating generations is therefore no limited principle, strange and anomalous, applying only to a few Radiata. It embraces under its scope, the vegetable kingdom, and it is but another instance of identity in the laws of growth in the two departments of life."[52]

Herbert Spencer read William B. Carpenter's *General and Comparative Physiology*, and he almost certainly also read some of

Carpenter's *Principles of Human Physiology*. Precisely how many of Carpenter's other writings were known to Spencer, either by reading or reputation, would be difficult to determine. Whether or not the above-mentioned article by Carpenter and Dana had any influence upon Spencer, for example, is unknown. By bringing together the methodological and the biological meanings of analogy, however, Carpenter established a link between German *Natürphilosophie* and English inductive philosophy. Equally important, Carpenter presented this synthesis in developmental terms. In reading Carpenter, Spencer was informed – not only about von Baer's embryological laws – but that biologists as well as physicists were capable of making great scientific generalizations, and that a principle tool in the making of such generalizations was the discovery of analogies. If he read Carpenter's physiological writings carefully, Spencer would also have learned, more specifically, that analogies could serve to verify and extend the laws of biological development.

HERBERT SPENCER

No single critical year marked Spencer's intellectual development, but for Spencer the 1850s was a crucial decade. In the ten years after December, 1850, when Spencer completed *Social Statics: or, The Conditions Essential to Human Happiness Specified, and The First of Them Developed*, Spencer was transformed from a classical political economist whose major concerns were issues like free trade, railway legislation and sanitary policy to the philosopher of evolution and social science. Most of Spencer's ideas received their first publication in one of the Victorian reviews. In 1852 the editorship of the *Westminster Review* was assumed by the publisher of Herbert Spencer's *Social Statics*, and the new editor proclaimed that his editorial policy would be to seek "recognition of the Law of Progress". The editor had Spencer in mind; and for Spencer himself the result was a burst of productivity. Spencer's contributions to the *Westminster Review* during the 1850s included "The Theory of Population" (1852); "Progress, its Law and Cause" (1857); and "Social Organism" (1860). In these papers Spencer first announced his most all-embracing analogies – between biological evolution and social progress and between the structure of natural organisms and society.

At the time of these articles, Spencer was a member of the small circle of freethinkers that included the authoress Mary Ann Evans, alias George

Elliot, and George Henry Lewes. Friendship between Lewes and Spencer was conducive to the scientific interests of both men, and it was at the prodding of Mary Ann Evans that Spencer finally read some of Comte in the original. Spencer wrote in his *Autobiography* that he met Lewes for the first time in the spring of 1850 and that this meeting was responsible for introducing Lewes, then known only for his literary work, into the field of biology.[53] Somewhat later, Lewes reciprocated by introducing Spencer to the biological ideas of Henri Milne-Edwards, an acquaintance ultimately to prove significant for Spencer's concept of the social organism. Both Herbert Spencer and George Henry Lewes had been interested in phrenology, which claimed to make the science of the mind scientific, and during the 1850s both men became involved in the evolutionary controversies that followed the anonymous publication of Robert Chambers's *Vestiges of Creation.*

During this crucial decade Spencer not only became interested in biology but he also developed a strong belief in the unity of the sciences. At first this belief may have been largely philosophical, but it was soon supported by Spencer's scientific reading. George Henry Lewes probably told Spencer of his interest in German philosophy and *Natürphilosophie.* However, Spencer's belief in unity was not due only to *Natürphilosophie,* which, in fact, Spencer on occasion severely criticized.[54] At mid century, some observers hoped for a unification of the sciences based upon the doctrine of the conservation of energy; others anticipated the same thing as the eventual outcome of a rejection of special vital forces and powers in biology. Herbert Spencer alluded in his *Autobiography* to the influence of these various unifying impulses upon his own thinking in the 1850s by writing that "the time was one at which certain all-embracing scientific truths of a simple order were being revealed."[55] In the same passage he mentioned, in particular, the importance of William Groves's *The Correlation of Physical Forces,* a book that in 1846 sought to establish the unity of the fundamental "forces" of nature, among which Grove included heat, light, and electricity. These unifying tendencies within science seemed to Spencer entirely consistent with the great generalizations that he believed were being discovered by biologists. They were also consistent with Spencer's belief in the explanatory power of analogies.

It is possible to trace in some detail Spencer's growing faith during the 1850s in the analogical method, which went hand-in-hand with Spencer's emerging conviction of an analogy between society and the individual organism. Already in his *Social Statics*, Herbert Spencer wrote that "We commonly enough compare a nation to a living organism. We speak of 'the body politic', of the functions of its several parts, of its growth, and of its diseases, as though it were a creature. But we usually employ these expressions as metaphors, little suspecting how close is the analogy, and how far it will bear carrying out. So completely, however, is a society organized on the same system as an individual being, that we may almost say there is something more than analogy between them."[56] In 1850 Spencer supported this metaphor by appeal to Richard Owen's "principle of vegetative repetition" – the idea (in Spencer's paraphrase) that "creatures of inferior type are little more than aggregations of numerous like parts", in contrast to higher animals, which contain a hierarchy of parts, some of which are few in number.[57] Spencer suggested that the differences described by Owen between lower and animals paralleled the differences between lower and higher human societies. Owen had discussed his principle of vegetative repetition in his *Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals*, which were delivered at the Royal College of Surgeons in 1843. In the published version of these lectures Owen described his views on vegetative repetition in the same concluding remarks in which he stated that the Invertebrata (and only the Invertebrata) fully illustrated the "analogy" between plants and animals "in the modes of continuing the species."[58]

Some of Spencer's views on the propriety of analogy in science were expounded in a paper published in 1854 entitled the "Genesis of Science".[59] Frustrated at being considered a disciple of Auguste Comte, Spencer in this paper rejected Comte's contention that positive knowledge of complex phenomena must follow and build upon the prior scientific understanding of simpler phenomena.[60] Spencer instead claimed that the sciences were all coeval and had influenced one another throughout their history. Six years before he wrote in his paper on the social organism that "through all its ramifications society is a growth and not a manufacture", Spencer applied the embryological metaphor to the history of science. In the "Genesis of Science", Spencer declared:

Anatomists and physiologists now find that the real natures of organs and tissues can be understood only by tracing their early evolution; and Prof. [Richard] Owen teaches that the affinities between existing genera can be satisfactorily made out only by examining the fossil genera to which they are allied. Well, is it not clear that the like must be true concerning all things that undergo development? Is not science growth; and must not science, too, have its embryology? And must not the neglect of its embryology lead to a misunderstanding of the principles of its evolution and of its existing organization?[61]

Spencer argued in his paper on the genesis of science that the interactions between the sciences were at the level of analogy:

Not only do sciences affect one another [directly], but they also affect each other indirectly. Where there is no dependence, there is yet analogy – *equality of relations*; and the discovery of relations subsisting amongst one set of phenomena constantly suggests a search for the same relations amongst another set.[62]

Spencer's definition of analogy as an "equality of relations" was derived from contemporary usage. In his *System of Logic*, John Stuart Mill noted that Analogy "sometimes stands for arguments which may be examples of the most rigorous Induction. Archbishop [Richard] Whately, for instance, following [James] Ferguson and other writers, defines Analogy conformably to its primitive acceptation, that which was given to it by mathematicians, Resemblance of Relations."[63] Mill also wrote that it was usual "to extend the name of analogical evidence to arguments from any sort of resemblance provided they do not amount to a complete induction: without peculiarly distinguishing resemblance of relations."[64] The year after the publication of his article on the genesis of science, Spencer also defined analogy as an equality of relations in his *Principles of Psychology*.

At the same time that Herbert Spencer was writing about analogy as the "equality of relations", he was also becoming increasingly familiar with biology. Richard Owen certainly contributed to Herbert Spencer's awareness of the comparative anatomists' use of the term "analogy" (as well as to some other topics) but Spencer was more profoundly indebted to the biological views of William B. Carpenter than to those of Owen. Indeed, considering the degree to which Carpenter's own stature was growing during the 1850s, Spencer could hardly have escaped his influence. There was more significance to Spencer's indebtedness to Carpenter, however, than is revealed simply by noting Carpenter's growing prominence; in using Carpenter rather than Owen as his primary biological authority, Spencer repudiated a static biological science whose roots lay in comparative anatomy, and he embraced instead a developmental biological science whose roots lay in physiology. Spencer acknowledged his indebtedness to Carpenter's influence on several occasions. Most notably, Spencer wrote in his *Autobiography* that in perusing an edition of Carpenter's *Principles of Physiology, General and Comparative* in order to write a notice for the *Westminster Review*, "I came across [Karl Ernst] von Baer's formula expressing the course of development through which ever plant and animal passes – the change from homogeneity to heterogeneity."[65] Spencer thus read Carpenter's textbook on general physiology for the first time sometime during 1851, at the very beginning of his transition from political economist to evolutionist. In all likelihood, Spencer also encountered Carpenter's textbook on human physiology only shortly later. Significantly, for Herbert Spencer's interests, Carpenter interpreted von Baer's embryological laws as indicative of a general law of development and progress. In his *Principles of Human Physiology*, the fifth edition of which was published in 1852, for example, Carpenter stated that von Baer's embryological laws showed that man is subject "to that great law of progress *from the general to the special*, which is equally manifested in the development of every other organized being.[66] Apparently, Spencer did not immediately seize upon what he would later consider to be the full implications of von Baer's laws. But in 1857, in his paper "Progress, its Law and Cause", Spencer adopted von Baer's formulation of the embryological laws as his own definition of progress and evolution: movement from a state of undifferentiated homogeneity to a state of differentiated heterogeneity.[67]

Herbert Spencer's acquaintance with Carpenter's physiological writings left other marked traces upon Spencer's intellectual development. Although not generally recognized by historians, and not directly relevant to Spencer's views on analogy, one important legacy of Herbert Spencer's reading of William Carpenter must have been Herbert Spencer's increased confidence in the inheritability of acquired characteristics, an idea that became a crucial part of Spencer's evolutionary moral science in Spencer's *Principles of Psychology*, first published in 1855. Except for Herbert Spencer himself, Carpenter was perhaps the most vocal English proponent of the theory of the inheritance of acquired characteristics. The inheritance of acquired characteristics, in fact, occupied an ever-increasing role in Carpenter's thought, beginning with the publication in 1839 of the first edition of Carpenter's *Principles of General and Comparative Physiology*.[68] Many early nineteenth-century writers, of course, believed in the doctrine of the inheritance of acquired characteristics: it was associated with the name of Jean-Baptiste Lamarck, and it was accepted by many phrenologists. Spencer knew of the phrenologists claim exercising one's mental organs could produce inheritable changes in the brain. Carpenter's discussion of the inheritability of acquired characteristics, however, was particularly significant for Spencer for the reason that Carpenter placed inheritability within a physiological and developmental context. Also significant was the fact that Carpenter's belief in the inheritance of acquired characteristics allowed him – as it would allow Spencer – to foresee a progressive mental development of the human race.

Even if Herbert Spencer had been unaware of the analogical method of reasoning before reading Carpenter's writings (which he was not), he could not have remained similarly unaware afterwards. Yet, Carpenter's physiological writings were not enough, by themselves, to lead Spencer to the conclusion that society was not only metaphorically *like* an organism but could be considered analogically identical to an organism – and therefore, in fact, *was* an organism. Carpenter discussed the development of "organized beings" from the very simplest to the most complex; but his account stopped with man and did not extend to the consideration of society as a type of organization. Although Carpenter had a side interest in psychology and anthropology, he was primarily a physiologist; there is no evidence that he had great interest in political economy, ethical philosophy, or other broad social questions. It is in this regard that Herbert Spencer's own background as a political economist was of significance. Ironically, two biological concepts by means of which Herbert Spencer extended the developmental process to encompass the "social organism" were originally analogies borrowed by biology from political economy.[69] One such analogy, between silver and gold in the state and the blood corpuscles in the body, was based upon similarity of function. From the chemist Justus Liebig's *Familiar Letters on Chemistry*, a book written in 1851, Spencer quoted, "Silver and gold have to perform in the organism of the State the same function as the blood corpuscles in the human organism. As these round discs, without themselves taking an immediate share in the nutritive process, are the medium, the essential condition of the change of matter, of the production of the heat, and of the force by which the temperature of the body is kept up and the motions of the blood and all the juices are determined, so has gold become the medium of all activity in the life of the State."[70]

Spencer's extension of the laws of biological development to society was most facilitated by the concept introduced in Adam Smith's *An*

Inquiry into the Nature and Causes of the Wealth of Nations; namely, the "division of labor". Using the phrase, "division du travail physiologique," the French zoologist Henri Milne-Edwards applied the idea of the division of labor to explain the difference between simple and complex animals. According to Milne-Edwards, the organs of simple animals "may be compared to a workshop, where the workmen are employed in the execution of similar labors, and where consequently then their number would influence the quantity, but not the nature of the products."[71] The organization of higher animals, however, revealed what Milne Edwards thought was akin to the division of labor in society: individual organs differing to an increasing extent in both structure and function. In his *Elemens de Zoologie* Milne-Edwards wrote that "the principle which seems to have guided nature in the perfection of beings is, as we see, precisely the one which has had the greatest influence upon the progress of human industry: *the division of labor.*"[72] English writers, including both Herbert Spencer and Charles Darwin, usually referred to Milne-Edwards's idea as the "physiological division of labor."[73]

Robert J. Richards has noted that Spencer appears to have first used Milne-Edwards' concept of the social division of labor in his 1852 article for the *Westminster Review* entitled "A Theory of Population, Deduced from the General Law of Animal Fertility".[74] In this article, Spencer also reviewed William B. Carpenter's *Principles of Physiology, General and Comparative*; indeed, well over half of Spencer's footnotes to this article were to either the third or the fourth edition of Carpenter's book. It is thus clear that Spencer began thinking about both Carpenter's physiological ideas and Henri Milne-Edwards concept of the physiological division of labor at very nearly the exact same time. Spencer again cited the implications of Milne-Edwards's "physiological division of labor" in 1854 in his paper on the "Genesis of Science". "The fact is," Spencer wrote in that article, "that the division of labour in science, like the division of labour in society, and like the 'physiological division of labour' in individual organisms, has been not only a specialization of functions, but a continuous helping of each division by all the others, and of all by each."[75] In his 1860 article on the "Social Organism" Spencer wrote that the "analogy between the economical division of labour and the 'physiological division of labour' is so striking, as long since to have drawn the attention of scientific naturalists."[76]

In his article on the social organism, Herbert Spencer articulated three "parallelisms" that, he maintained, demonstrated the analogy between society and an individual organism. The correspondence between Herbert Spencer's three parallelisms and William B. Carpenter's description of the growth and development of organisms in the latter's *Principles of General and Comparative Physiology* and *Principles of Human Physiology* was striking. Both Carpenter and Spencer emphasized the factor of growth, and both employed the word "mutual dependence" to describe the result of development among complex organisms. Carpenter stressed the importance of growth and development – but he also stressed the importance of distinguishing between "growth" and "development". Taking a point-of-view that would be echoed by Herbert Spencer, Carpenter argued that mere growth was eventually succeeded by actual development. In his *Principles of Human Physiology*, for example, Carpenter wrote that "The evolution of the complete organism from its germ . . . does not consist of mere growth; for by such a process nothing would be produced but an enormous aggregation of simple cells, possessing little or no mutual dependence, like those which constitute the shapeless masses of the lowest Algae. In addition to increase, there must be *development*, that is, a passage to a higher condition, both of form and structure, so that the part in which this change takes place becomes fitted for some special function."[77] Herbert Spencer wrote:

> Societies agree with individual organisms in three conspicuous peculiarities: –
>
> 1. That commencing as small aggregations they insensibly augment in mass; some of them reaching eventually perhaps a hundred thousand times what they originally were.
>
> 2. That while at first so simple in structure as to be almost considered structureless, they assume, in the course of their growth, a continually increasing complexity of structure.
>
> 3. That though in their early undeveloped state there exists in time scarcely any mutual dependence of parts, these parts gradually acquire a mutual dependence, which becomes at last so great, that the activity and life of each part is made possible only by the activity of life of the rest.[78]

Herbert Spencer's claim that society was akin to a natural organism was to have a profound impact upon nineteenth-century social thought. For Herbert Spencer, himself, the view that "society is a growth and not a manufacture" reinforced the laissez-faire sentiments of Spencer's *Social Statics*, and indeed of much of the rest of classical political economy.[79] The laws of political economy, which many already claimed were rooted in nature, received a new legitimation as aspects of the more universal laws of development. In his paper on the social organism, Spencer made explicit the political implications of considering the division of labor to be the product of inevitable and universal natural law. Society, he wrote, arose as natural process, and not through "legislative guidance". The division of labor, he contended, was not the product of social planning but of the inevitable development of society as a whole that occurred even as individuals pursued their own interests.

> The whole of our industrial organization, from its most conspicuous features down to its minutest details, has become what it is, not only without legislative guidance, but, to a considerable extent, in spite of legislative hindrances. It has arisen under the pressure of human wants and activities. While each citizen has been pursuing his individual welfare, and none taking thought about division of labour, or indeed conscious of the need for it, division of labour has yet been ever becoming more complete.[80]

For the history of social thought, Herbert Spencer's concept of society as an organism transcended in importance even its role as a confirmation of classical political economy. Spencer's naturalistic analogy was to give rise to a host of late nineteenth-century attempts to create a social science through the use of biological analogy.[81] Still more important, however, the concept of society as an organism was a half-way step between the world of Adam Smith at the end of the eighteenth century and that of the sociologist Émile Durkheim at the beginning of the twentieth. Smith's *An Inquiry into the Nature and Causes of the Wealth of Nations* implied that economic development was affected by social organization, but it was left for Herbert Spencer's naturalistic analogy to broaden the focus from economic progress to social organization itself. Herbert Spencer's claim that society was an organism meant that society was a *thing*, and that this thing was greater than the sum of its parts. The concept of the "social organism" thus implied the need for study of society apart from the study of the individual – in other words, to a sociology independent of both biology and psychology. Herbert Spencer, although he was to write as a sociologist, did not follow along this path; for Spencer the individual and society remained connected through the universal laws of development. But Spencer's analogy suggested that society, like an individual, has a life of its own that is independent of its constituents. Spencer's naturalistic analogy of society thus led directly to its own non-naturalistic antithesis: to Émile Durkheim's concept of society as a thing *sui generis*.

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NOTES

[1] [Herbert Spencer]: "The Social Organism", *Westminster Review*, 1860, **73**: 51–68.
[2] *Ibid.*, p. 56.
[3] *Ibid.*, p. 53.
[4] See especially J.D.Y. Peel: *Herbert Spencer: The Evolution of a Sociologist* (New York: Basic Books, 1971), pp. 166–191.
[5] J.D.Y. Peel, "Herbert Spencer" in: *Dictionary of Scientific Biography* (New York: Charles Scribner's Sons, 1975), vol. 12, p. 570.
[6] Herbert Spencer: *An Autobiography*, (New York: D. Appleton and Company, 1904), vol. 1, pp. 402–403.
[7] Robert J. Richards: *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior* (Chicago: The University of Chicago Press, 1987 – Science and its Conceptual Foundations, David L. Hull, editor), esp. pp. 295–330. Richards also notes (p. 557) that other factors involved in Spencer's development were "Carpenter's physiology, Lamarck's theory via Lyell, Hamilton's neo-Kantianism, and Darwin's theory of community selection."
[8] [Herbert Spencer]: "The Social Organism", p. 54.
[9] Peel (n. 5 supra) notes that Spencer's way "had been cleared by Spencer's reading of K.E. von Baer's work on embryology and H. Milne-Edwards' theme of 'the physiological division of labor'." Also see Robert J. Richards (n. 7 supra), esp. pp. 267–274.
[10] John R. McCulloch: "Political Economy", *The Encyclopaedia Britannica or Dictionary of Arts, Sciences, and General Literature*, seventh edition, (Edinburgh: Adam and Charles Black, 1842), vol. 18, p. 260. Most of this article was the same as McCulloch's *Britannica* article of 1824. Evidently, McCulloch realized that this claim for political economy was too strong, because it was omitted in J.R. McCulloch: *The Principles of Political Economy; with some inquiries respecting their application, and a sketch of the rise and progress of the science*, a work based on McCulloch's *Britannica* article that was printed separately in 1825. The debate over the relative role of induction and deduction in political economy needs further study, especially with reference to the broader context of English discussions of scientific methodology in the early nineteenth century.
[11] John Stuart Mill: *A System of Logic, Ratiocinative and Inductive. Being a Connected View of the Principles of the Evidence and the Methods of Scientific Investigation* [1843], (New York: Harper and Brothers, 1848), Book VI, Chap. VII, pp. 550–555.
[12] *Ibid.*, Book VI, Chap. VII, p. 552.
[13] Auguste Comte: *The Positive Philosophy of Auguste Comte*, freely translated and condensed by Harriet Martineau (New York: D. Appleton and Co.; London: J. Chapman, 1853), p. 5.
[14] John Stuart Mill: *A System of Logic*, p. v.
[15] Francis Wayland: *The Elements of Moral Science*, 8th ed. (Boston: Gould, Kendall, and Lincoln, 1839), p. 23–25.
[16] James Bonar: *Philosophy and Political Economy in Some of Their Historical Relations*, 3rd ed. (London: George Allen and Unwin, 1922), p. 140.
[17] George Combe: *The Constitution of Man Considered in Relation to External Objects* [1828], facsimile reproduction, intr. Eric T. Carlson (Delmar [New York]: Scholars' Facsimiles & Reprints, 1974), "Preface", p. vii–viii.
[18] For phrenology see Roger Cooter: *The Cultural Meaning of Popular Science:*

Phrenology and the Organization of Consent in Nineteenth-century Britain (Cambridge: Cambridge University Press, 1984).

[19] Henry Thomas Buckle: *History of Civilization in England*, from the second London edition (New York: D. Appleton & Co., 1898), vol. 1, pp. 5–6.

[20] Richard Olson: *Scottish Philosophy and British Physics, 1750–1880* (Princeton, New Jersey: Princeton University Press, 1975).

[21] "Analogy" in *Encyclopaedia Britannica; or, a Dictionary of Arts and Sciences compiled upon a New Plan*, by a Society of Gentlemen in Scotland (Edinburgh: printed for A. Bell and C. Macfarquhar, 1771), vol. 1, p. 142.

[22] For a discussion of the context of Herschel's views on analogy see Richard Olson: *Scottish Philosophy and British Physics*, esp. 252–270.

[23] Newton's illustrations of his principle that the same effects should be assigned the same causes were: "As to respiration in a man and in a beast; the descent of stones in *Europe* and in *America*; the light of our culinary fire and of the sun; the reflection of light in the earth, and in the planets." Isaac Newton: *Sir Isaac Newton's Mathematical Principles of Natural Philosophy and his System of the World*, trans. Andrew Motte, rev. by Florian Cajori (Berkeley, California: University of California Press, 1960), p. 398.

[24] "Analogy" in *Encyclopaedia Americana. A Popular Dictionary of Arts, Sciences, Literature, History, Politics and Biography, A new edition; including a copious collection of original articles in American biography; on the basis of the seventh edition of the German Conversations-Lexicon*, Francis Lieber, assisted by E. Wigglesworth. (Philadelphia: Blanchard and Lea, 1857), vol. 1, p. 231.

[25] John Stuart Mill: *System of Logic*, Book III, Chap. 20, p. 333.

[26] *Ibid.*, Book V, Chap, 5, p. 492.

[27] John Theodore Merz: *A History of European Thought in the Nineteenth Century* (London: William Blackwood & Sons, 1904), vol. 2, p. 250.

[28] Quoted in [Pierre] Flourens: "Memoir of Geoffroy Saint-Hilaire", *Annual Report of the Board of Regents of the Smithsonian Institution* (Washington: George W. Bowman, 1861), p. 165. Quoted from Geoffroy Saint-Hilaire: "Dissertation sur les makis", *Magasin Encyclopédique*, 1796, **7**: 20.

[29] Quoted in Flourens, p. 168.

[30] Richard Owen: *Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals*, delivered at the Royal College of Surgeons, in 1843 (London: Longman, Brown, Green, and Longmans, 1843), pp. 374 and 379.

[31] Robert Macleay: *Horae Entomologicae: or, Essays on the Annulose Animals* (London: S. Bagster, 1821), vol. 1, part 2, p. 363. For the quinary theorists see Ernst Mayr: *The Growth of Biological Thought: Diversity, Evolution, and Inheritance* (Cambridge: Harvard University Press, 1982), pp. 202–203.

[32] J[ean] B[aptiste] Lamarck: *Zoological Philosophy: An Exposition with Regard to the Natural History of Animals*, trans. with an introduction by Hugh Elliot (London: Macmillan & Co., 1914), p. 29. Mayr (n. 31 supra) points out the historical significance of the distinction between analogy and affinity.

[33] William Swainson: *Preliminary Discourse On The Study of Natural History* (London: Longman Rees, Orme. Brown, Green & Longman, 1834), pp. 283–295.

[34] *Ibid.*, p. 282.

[35] William Whewell: *History of the Inductive Sciences*, third ed. [1857] (reprint London: Cass & Co., 1967), vol. 3, Book XVI, chap. 6, p. 295.
[36] Phillip F. Rebock: *The Philosophical Naturalists: Themes in Early Nineteenth-century British Biology* (Madison: University of Wisconsin Press, 1983).
[37] William B. Carpenter: "On Unity of Function in Organized Beings", *The New Edinburgh Philosophical Journal*, 1837, **23**: 92–114. Considering his contemporary reputation, William B. Carpenter has not received the systematic attention that he deserves. Standard biographical accounts are G[eorge] T[homas] B[ettany], "William Carpenter", *Dictionary of National Biography*, vol. 3, pp. 1075–1076; K. Bryn Thomas, "William Carpenter", *Dictionary of Scientific Biography*, vol. 3, pp. 87–89. Three recent articles by L.S. Jacyna provide additional information on Carpenter and his milieu – L.S. Jacyna: "The Physiology of Mind, the Unity of Nature, and the Moral Order in Victorian Thought", *British Journal for the History of Science*, 1981, **14**: 109–132; L.S. Jacyna: "The Romantic Programme and the Reception of Cell Theory in Britain", *Journal of the History of Biology*, 1984, **17**: 13–48; L.S. Jacyna: "Principles of General Physiology: The Comparative Dimension to British Neuroscience in the 1830s and 1840s", *Studies in the History of Biology*, 1984, **7**: 47–92.
[38] William B. Carpenter: "On the Unity of Function in Organized Beings", p. 94.
[39] William B. Carpenter: *Principles of General and Comparative Physiology, Intended as an Introduction to the Study of Human Physiology and as a Guide to the Philosophical Pursuit of Natural History* (London: John Churchill, 1839), p. 163. Carpenter took the quotation from George Cuvier: *Discours sur les révolutions de la surface du globe* (Paris, 1825).
[40] W.B. Carpenter: *Principles of General and Comparative Physiology*, 1839, p. 163.
[41] *Ibid.*
[42] *Ibid.*, dedication page.
[43] Quoted in William B. Carpenter: *Principles of General and Comparative Physiology*, p. 134. The quotation was from John Herschel: *Preliminary Discourse on the Study of Natural Philosophy* (London: 1830), p. 37.
[44] At least Powell's claims for the importance of analogy in induction became widely known during the late 1830s and 1840s. For Powell see Robert Fox: "Baden Powell", *Dictionary of Scientific Biography*, vol. 11, pp. 115–116.
[45] Baden Powell: *On the Nature and Evidence of the Primary Laws of Motion* (Oxford: The Ashmolean Society, 1837).
[46] *Ibid.*, pp. 55–56.
[47] Quoted in William B. Carpenter: *Principles of General and Comparative Physiology*, 1839, p. 163. The quotation was from Baden Powell: *The Connexion of Natural and Divine Truth; or, The Study of the Inductive Philosophy Considered as Subservient to Theology* (London: John W. Parker, 1838), p. 33.
[48] Baden Powell: *Connexion of Natural and Divine Truth*, p. 26.
[49] William B. Carpenter: *Principles of General and Comparative Physiology*, 1839, p. 11.
[50] Dov Ospovat, "The Influence of Karl Ernst von Baer's Embryology, 1829–1859: A Reappraisal in Light of Richard Owen's and William B. Carpenter's Paleontological Application of 'von Baer's Law'", *Journal of the History of Biology*, 1976, **9**: 10.
[51] William B. Carpenter: "On Unity of Function of Organized Beings", pp. 92–93.

[52] William B. Carpenter and James D. Dana, "On the Analogy between the mode of Reproduction in Plants and the 'Alternation of Generations' observed in some Radiata", *Edinburgh New Philosophical Journal*, 1851, **50**: 266–268.
[53] Herbert Spencer *Autobiography* (New York: D. Appleton & Co., 1904), vol. I, p. 435–439.
[54] See, for example, Spencer's criticisms of *Natürphilosophie* in [Herbert Spencer]: "The Genesis of Science", *British Quarterly Review*, 1854, **20**: 108–162. In discussing Oken's cosmological ideas, Spencer wrote (p. 116) that "to comment on them would be nearly as absurd as are the propositions themselves."
[55] Herbert Spencer: *Autobiography*, vol. 2, p. 15.
[56] Herbert Spencer: *Social Statics: or, The Conditions Essential to Human Happiness Specified and The First of Them Developed* [1851] (reprint, New York: Augustus M. Kelley, 1969), p. 448.
[57] *Ibid.*, p. 451.
[58] Richard Owen: *Lectures on Comparative Anatomy and Physiology*, 1843, p. 364.
[59] [Herbert Spencer]: "The Genesis of Science", *British Quarterly Review*, 1854, **20**: 108–162.
[60] Sidney Eisen: "Herbert Spencer and the Spectre of Comte", *The Journal of British Studies*, 1967, **7**: 48–67.
[61] [Herbert Spencer]: "The Genesis of Science", p. 114.
[62] *Ibid.*, p. 153.
[63] John Stuart Mill: *System of Logic*, Book III, Chap. XX, p. 332.
[64] *Ibid.*
[65] Herbert Spencer: *Autobiography*, vol. 1, p. 445.
[66] William B. Carpenter: *Principles of Human Physiology* (Philadelphia: Blanchard and Lea, 1853), p. 986.
[67] [Herbert Spencer]: "Progress: Its Law and Cause", *Westminster and Foreign Quarterly Review*, 1857, **67**: 445–485.
[68] Carpenter's interest in the inheritance of acquired characteristics proved to be long-standing. See William B. Carpenter: "On the Hereditary transmission of Acquired Psychical Habits", *Contemporary Review*, 1873, **21**: 294–314, 778–795, 867–885. Another point of contact between Carpenter and Spencer lay in Carpenter's interest in the law of conservation of energy as a unifying principle in nature. See Vance Hall: "The Contribution of the Physiologist William B. Carpenter (1813–1885) to the Development of the Principles of the Correlation of Forces and the Conservation of Energy", *Medical History*, 1979, **23**: 129–155.
[69] Herbert Spencer noted this irony himself. In 1873, Spencer described what he called the "reciprocal relationship between the sciences of biology and sociology. Referring particularly to the concept of the division of labor, Spencer wrote, "We have but to glance back at its progress, to see that Biology owes the cardinal idea on which we have been dwelling, to Sociology; and that having derived from Sociology this explanation of development, it gives it back to Sociology greatly increased in definiteness, enriched by multitudiness illustrations, and fit for extension in new directions." See Herbert Spencer: "The Study of Sociology", *The Contemporary Review*, 1873, **22**: 331.
[70] Justus Liebig: *Familiar Letters on Chemistry* [London, 1851, p. 466], abbreviated quotation in [Herbert Spencer]: "Social Organism," p. 63.

[71] Henri Milne Edwards: *Outlines of Anatomy and Physiology*, trans. J.F.W. Lane (Boston: Charles C. Little, 1851), p. 11.
[72] *Ibid.*, p. 12.
[73] For a discussion of the influence of Milne-Edwards upon Charles Darwin see Camille Limoges: "Darwin, Milne-Edwards et le principe de divergence", *Actes XIIe Cong. Int. Hist. Science*, 1968, **8**: 111–113.
[74] Richards, (n. 7 supra), p. 271. See [Herbert Spencer]: "A Theory of Population, deduced from the General Law of Animal Fertility", *Westminster Review*, 1852, **57**: 259–260.
[75] [Herbert Spencer]: "Genesis of Science", p. 126.
[76] [Herbert Spencer]: "The Social Organism", *Westminster Review*, 1860, **73**: 59.
[77] William B. Carpenter: *Principles of Human Physiology, with their chief applications to psychology, pathology, therapeutics, hygiène, and forensic medicine* (Philadelphia: Blanchard and Lea, 1853), pp. 547–48.
[78] [Herbert Spencer]: "Social Organism", p. 54.
[79] *Ibid.*, p. 53.
[80] *Ibid.*, p. 52.
[81] An early enumeration of organismic analogies in social thought is provided in Lester Ward: "Contemporary Sociology", *The American Journal of Sociology*, 1902, **7**: 480.

S. S. SCHWEBER

9. DARWIN AND THE AGRONOMISTS: AN INFLUENCE OF POLITICAL ECONOMY ON SCIENTIFIC THOUGHT

The *Origin of Species* was the culmination of Darwin's theorizing of the previous twenty years. Its unique role in delineating the subsequent debates over all aspects of evolution accounts for an enduring interest in the construction of the *Origin* and in the intellectual and social factors that helped shape its final form.[1] Of especial theoretical importance are the dynamical explanations that Darwin advanced in the *Origin*. These can be traced back at least to January 1839, when, in his fourth notebook on the transmutation of species, the E notebook, Darwin expressed a view of dynamics based on his notion that nature's dynamical equilibrium maximizes the amount of life per unit area and that diversity is a way of accomplishing such maximization. One of the sources of this approach was the literature on scientific agriculture, which for the social scientists of the eighteenth and early nineteenth centuries formed an important part of or adjunct to the subject of political economy.

From 1837 on, Darwin was constantly trying to answer the question: "What constitutes an explanation in biology – more particularly, a *dynamical* explanation that identifies and refers to causes, *verae causae*?"[2] The models presented by the physical, chemical, and geological sciences – as well as the historical development of these sciences – were important factors helping him to arrive at his own formulation.[3] But in answering his question, Darwin abandoned the Newtonian model of dynamical explanation in important respects and came to a novel concept of dynamics for biological systems. He recognized that living systems were infinitely more complicated than Newton's planetary system. Biological "elements" had characteristics that were changing in time; they had a history. All the interactions of organisms, whether with one another or with the environment, were non-additive and non-instantaneous and exhibited memory. It was the ahistorical nature of the objects with which physics dealt that gave the Newtonian scheme the possibility of a simple, mathematical description. It was precisely the *historical* character of living objects that gave biological phenomena their unique and complex features.

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 305–316.

Certainly one aspect of the staggering entry in the E notebook dated January 1839 is quite explicit:

The enormous *number* of animals in the world depends of their varied structure & complexity. – hence as the forms became complicated, they opened *fresh* means of adding to their complexity. – but yet there is no *necessary* tendency in the simple animals to become complicated although all perhaps will have done so from the new relations caused by the advancing complexity of others. – It may be said, why should there not be at any time as many species tending to dis-development . . . , my answer is because, if we begin with the simplest forms & suppose them to have changed, their very changes tend to give rise to others. – . . . I doubt not if the simplest animals could be destroyed, the more highly organized would soon be disorganized to fill their places. –

The geologico-geographico changes must tend sometimes to augment & sometimes to simplify structures. Without enormous complexity, it is impossible to cover *whole* surface of world with life. – for otherwise a frost if killing the vegetables in one quarter of the world would kill all . . . it is quite clear that a large part of the complexity of structure is adaptation. . . . Considering the Kingdom of nature as it now is, it would not be possible to simplify the organization of the different beings, (all fishes to the state of the Ammocoetus, Crustacea to – ? &c) without reducing the number of living beings – but there is the strongest possible [tendency] to increase them, hence the degree of development is either stationary or more probably increases.[4]

This entry presents a view of organisms as entities that have a history and a memory. Only by virtue of having obtained a degree of complexity can organisms add further complexity. Although there is no necessary tendency in the simple animals to become complicated, "new relations caused by the advancing complexity of others" will induce complexity to evolve, and complexity means diversity. Change entrains change. In addition, it is the interaction *among* organisms that gives rise to further complexity, that is, to novelty and diversity.

The end of the entry is equally impressive. Since there is the strongest possible tendency to increase the number of living beings[5] since every organism tries to increase in a Malthusian fashion – the development probably increases. Darwin's phrase "the strongest possible tendency to increase" marks a significant deduction from the Malthusian principle. It will be stated in *Natural Selection* as follows:

Every single organism may be said to try its utmost to increase (geometrically) therefore there is the strongest possible power tending to make each site to support as much life as possible.[6]

This maximization principle – "to try its utmost to increase" or "the strongest possible tendency to increase" – was an important insight. Darwin suggests that the dynamics of the situation, though very complicated – geometric increases through reproduction, intense inter- and intra-specific competition, interaction with site and climate – result in a dynamical equilibrium such that the greatest amount of life possible is supported in a given area of the surface of the earth. A comparison with the complicated processes that determine the topography of the earth's surface is revealing. Lyell had argued that the complicated effects of igneous and aqueous action combine to produce changes such that the net amount of surface area of land masses over the globe is constant. Darwin's principle of the maximum amount of life, however, represents something novel. What Darwin asserts emerged from the dynamics is not a conservation law but a *maximization principle*. Yet in 1839 this principle operated under the Lyellian constraint that globally the number of species is conserved; individuals may increase, but the total number of species is constant.[7]

The entry in the fourth transmutation notebook (E 95–97) must be augmented by two jottings recorded in the second, or C, notebook. In one of them Darwin suggests:

The end of formation of species & genera is probably to add to quantum of life possible with certain preexisting laws. – If only one kind of plants not so many. – [8]

On the next page (later excised), he asserts:

The *quantity of life* on planet at different periods depends on relations of desert, open ocean, &c. This probably on long average equal quantity, 2° on relation of heat & cold, therefore probably fewer now than formerly. The *number of forms depends* on the external relations (a fixed quantity) & on subdivision of stations & diversity, this perhaps on long average equal.[9]

When coupled with these, E 95–97 contains many of the insights which will later go into the principle of divergence, although in 1838–1839, when these entries were written, there were indeed many "preexisting" constraints: conservation of species, conservation of land masses, and adaptation to *stations* regarded as geographical localities. But the notion that diversification could increase the "quantum of life" became from that time on one of Darwin's theoretical assumptions. For example, it is given as an explanation for the paucity of species and the diversity of genera in Coral Islets in the spring of 1844:

Explanations of fewness of species and diversity of genera, I think must be partly accounted for the plants groups could subsist in greater numbers, and interfere less with each other. This must be explanation of Arctic Regions – How are alpine plants. Several Genera?[10]

The note is very explicit: In any locality the largest amount of plant life will be supported if there is diversity, for then "the plants groups [will] interfere less with each other."

There were several sources for Darwin's characterization of the equilibrium as one which maximizes the amount of life per unit area. One of the most important was the literature on scientific agriculture, including the related writings on political economy.[11] Agriculture was a central concern in all discussions of political economy, starting with Quesnay and the physiocrats and with Adam Smith. A good example is the influential series of lectures on *Political Economy* that Dugald Stewart delivered in the first decade of the nineteenth century.[12] In these lectures he gave a critical overview of political economy at the turn of the century.[13] As befitted his Scottish training and outlook, Stewart's presentation was sensitive to the sociological aspects of the subject matter and offered illuminating comparisons with French and Continental practices. He began by indicating the role and interrelation of agriculture[14] and manufactures in the economy of a nation. A disciple of Adam Smith, he stressed the *self-regulating* character of the free market:

In the midst of this conflict of contending interests and prejudices, it is the business of the Political Economist to watch over the *concerns of all*, and point out to the Legislator the danger of listening exclusively to claims founded in local or in partial advantages, to remind him that the pressure of a temporary scarcity brings along with it in time its own remedy, while an undue depression of prices may sacrifice to a passing abundance years of future prosperity; – above all, to recommend to him such a policy, as by securing in ordinary years a regular *surplus*, may restrain the fluctuation of prices within as narrow limits as possible. . . . [15]

In Part I of his *Political Economy*, Stewart was concerned with the relation of the size of the population of a country to its agricultural practices. The central problem for Stewart was to discover what kind of agriculture would maximize output so as to support the largest and best-fed population.[16] Drawing on Arthur Young's *Political Arithmetic* (1774), Stewart noted that

in the French system of husbandry . . . much the greatest part of the farm is arable; – the meadow and pasture being very trifling, except in spots that cannot otherwise be applied, and near great towns. Thus very little cattle can be kept except for tillage; in very many farms no other. Here we find manuring cut off at once, almost completely, and consequently the crops must be poor. Besides this, one-half or one-third of the land is fallow. . . . [17]

Stewart went on by quoting Young approvingly:

It must surely be evident to every one, that there is a great advantage to the *English* farmer, from corn and cattle being in equal demand, since he is thereby enabled to apply all his lands to those productions only to which they are best adapted; while, at the same time, the one is constantly the means of increasing the produce of the other.[18]

He continued by endorsing Young's conclusion that

where tillage and pasturage are properly combined, so as to have the farms from one-third to half of meadow or pasture; and the other two-thirds or half thrown into a proper course for the winter support of the cattle, such a farm will be found to feed more men than if it is all ploughed up, and as much wheat as is possible raised upon the French system.[19]

A further inquiry of relevance for maximal agricultural output concerned the size of farms. Arthur Young had also addressed this question and maintained the advantages of large farms,[20] but Stewart preferred the "general maxim" that "*The best size of farm is that which affords the greatest proportional produce, for the least proportional expense.*"[21]

The questions raised and the answers given by Dugald Stewart were surely known to Darwin. Josiah Wedgwood the younger ("Uncle Jos") was very much concerned with scientific agriculture, and James MacIntosh had an abiding interest in political economy. Darwin greatly respected both of them and had lengthy conversations with them during his visits to Maer.[22] In addition, when Darwin's father, Robert Waring Darwin, was studying at Edinburgh University he associated with Thomas Reid and Dugald Stewart, then the most eminent intellectuals of Scotland, and often visited Dugald Stewart's home.[23] Moreover, documentary evidence of Darwin's interest and reading in the work of Dugald Stewart dates from as early as August 1838, a time when, he says in his journal:

Read a good deal of various amusing books & paid some attention to metaphysical subjects.[24]

An entry made in the M notebook during August 1838 indicates that Darwin was studying the ideas of Adam Smith in the biographical memoir by Dugald Stewart:

Adam Smith (D. Stewart life of p. 27) says [sympathy] we can only know what others think by putting ourselves in their situation & then we feel like them – hence sympathy does not like Burke explain pleasure.[25]

Another entry in the M notebook, for the end of September or the beginning of October 1838, similarly refers to Stewart's biographical memoirs, which contain the lives of Thomas Reid and William Robertson in addition to that of Adam Smith:

D. Stewart [Smith] lives of Adam Smith. Read, etc. [Adam Smith, Reid, etc.] worth reading as giving abstract of Smith's views.[26]

And the second transmutation notebook – the C notebook – has a comparable entry probably written about the same time:

Du[gald] Stewart works & lives of Reid, Smith, & giving abstracts of their view.[27]

There are also undated references to Dugald Stewart, especially to his essays on the sublime and on taste, in the "Old and Useless Notes."[28] None of this evidence, of course, proves that Darwin read Stewart's *Political Economy*, but it does demonstrate his familiarity with Stewart and his interest in Stewart's ideas and in Stewart's formulations of the ideas of others. It also suggests that Stewart's views on agricultural economy were important in the eyes of the Darwins and Wedgwoods and others of their circle.

Furthermore, as is the case with many of Charles's investigations, his grandfather Erasmus Darwin had been there first. In 1800 Erasmus had written his *Phytologia*, subtitled *The Philosophy of Agriculture and Gardening*, which is a treatise on agricultural chemistry and the political economy of agriculture.[29] Charles had studied it during the very period when he was writing his transmutation notebooks.[30]

Darwin was undoubtedly also familiar with Humphry Davy's influential lectures on *Agricultural Chemistry* (1813). These contained many useful insights on the economics, that is to say, the dynamics, of agriculture. Thus, when discussing the yield from pastures, Davy indicated that nature

has provided, in all permanent pastures, a mixture of various grasses, the produce of which differs at different seasons. Where pastures are to be made artificially, such a mixture ought to be imitated: and, perhaps, pastures superior to the natural ones may be made by selecting due proportions of these species of grasses fitted for the soil which affords respectively the greatest quantities of spring, summer, latter-math [mowing], and winter produce. A reference to the details in the Appendix will show that such a plan of cultivation is very practicable.[31]

The Appendix is a seventy-page "Account of the Results of Experiments on the Produce and Nutritive Qualities of Different Grasses, and other Plants, used as the Food of Animals, instituted by John Duke of Bedford."[32]

Davy also inquired into the relation between the grasses or plants grown on the pasture, the animals that can be raised on these plants, and the nutritive and regenerative value (in terms of mineral and organic matter) of the manure excreted by these animals onto these same fields and plants. The intent was to discover which animals to raise and which grasses to grow in order to maximize the value of the output.[33] Davy conceived of his approach as a scientific procedure to achieve the steady state, that is, equilibrium, which maximized output in a manner consistent with nature's constraints.[34] Darwin followed the growing literature on this ecological approach to land management and animal husbandry after he moved to Down, and this scientific approach to agriculture probably assumed an even greater importance after Darwin purchased a farm in Lincolnshire in 1845 as an investment.

Agricultural chemistry as a scientific discipline was given a great stimulus by Justus Leibig's *Organic Chemistry in its Applications to Agriculture and Physiology*[35] (1840). Leibig had initially prepared this treatise as a report to the BAAS meeting of 1841. In the preface, addressed "To the British Association for the Advancement of Science", he explained:

> I have endeavoured to develop, in a manner correspondent to the present state of science, the fundamental principles of chemistry in general, and the laws of organic chemistry in particular, in their applications to agriculture and physiology. . . .

He went on to declare that

> perfect agriculture is the true foundation of all trade and industry – it is the foundation of the richest of states. But a rational system of agriculture cannot be formed without the application of scientific principles; for such a system must be based on an exact acquaintance with the means of nutrition of vegetables, and with the influence of soils and action of manure upon them.[36]

A companion volume on *Animal Chemistry, or Organic Chemistry in its Applications to Physiology and Pathology* (1842), presented Liebig's view of the process of nutrition in animals and in particular "the origin of animal excrements" and "the cause of their beneficial effects on the growth of vegetables."[37] Darwin's reading notebooks show that he read – or intended to read – Liebig's *Organic Chemistry in its Applications to Agriculture* in November 1841, for on November 21 he wrote: "Liebig's Agriculture – do."[38] Darwin also read Liebig's *Familiar Letters* (1943), first in 1844 and again in 1851.[39] The tenth letter dealt with the relation between agriculture and the growth of human population. Liebig's views were that the cultivation of our crops has ultimately no other object than the production of a maximum of those substances which are adapted for assimilation and respiration, in the smallest possible space. . . .

Cultivation is the economy of force [energy]. Science teaches us the simplest means of obtaining the *greatest* effect wth the *smallest* expenditure of power, and with given means to produce a maximum of force [energy].[40]

It should be noted that in Darwin's time this maximization approach had wide currency in Great Britain. In part it was a legacy of the Benthamite tradition, with its felicity calculus. But undoubtedly the most important reason for the popularity of the approach was the influence of Adam Smith and the school of political economy for which he was responsible.[41] It was Adam Smith's basic tenet that in a *laissez-faire*, politically uncontrolled economy, the efforts of each person to act in his own self-interest, i.e., to better himself, would result in that distribution of capital, labor, and land which maximized their respective returns by maximizing the value of the output to the public. Moreover, even though "every individual... intends only his own gain," he is often "led by an invisible hand to promote an end which was no part of his intention," and by "pursuing his own interest he frequently promotes that of the society more effectually than when he really intends to promote it."[42]

But the differences between Davy and Adam Smith ought to be stressed. For Davy it is an *externally* imposed maximization requirement, that the value of the output be greatest, which determines the equilibrium. In Adam Smith's position, the maximization is a *self-regulating, self-consistent* one. The same parallel, of course, exists between the artificial economy (read selection) and the natural economy (read selection) and is the reason why Darwin's project mirrors that of Adam Smith. Yet it was especially through reading the life of Smith by Dugald Stewart – who, together with Davy, may be numbered among the agronomists – that Darwin came to an understanding of at least some aspects of Smith's thought. And it was, at least in part, a familiarity with the ideas of Stewart, Davy, and Liebig regarding scientific agriculture that helped Darwin to formulate his own view of how, through diversity, a dynamical equilibrium in nature maximizes the amount of life per unit area. It can be asserted, therefore, that the agricultural branch of political economy influenced the thinking which led to the maximization principle in evolutionary biology.

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NOTES

[1] Silvan S. Schweber, "The Origin of the *Origin* Revisited", *Journal of the History of Biology*, 1977, **10**: 229–316; "The Genesis of Natural Selection – 1838: Some Further Insights", *BioScience*, 1978, **28**: 321–326; "The Young Darwin", *Journal of the History of Biology*, 1979, **12**: 175–192; "Early Victorian Science: *Science in Culture*", *Journal of the History of Biology*, 1980, **13**: 121–140; "Darwin and the Political Economists: Divergence of Character", *Journal of the History of Biology*, 1980, **13**: 195–289; "Demons, Angels, and Probability: Some Aspects of British Science in the Nineteenth Century", pp. 319–363 of Abner Shimony & Laszlo Tisza (eds.); *Physics as Natural Philosophy: Essays in Honor of Laszlo Tisza on His Seventy-Fifth Birthday* (Cambridge/London: The MIT Press, 1982); "Facteurs idéologiques et intellectuels dans la genèse de la théorie de la sélection naturelle", pp. 123–142 of Yvette Conry (ed.): *De Darwin au Darwinisme: science et idéologie* [Congrès International pour le Centenaire de la Mort de Darwin, Paris-Chantilly, 13–16 septembre 1982] (Paris: Librairie Philosophique J. Vrin, 1983); "The Wider British Context in Darwin's Theorizing", pp. 35–69 of David Kohn (ed.): *The Darwinian Heritage*. (Princeton: Princeton University Press, 1985).

[2] A cause is a "vera causa" if it can be shown (1) to be real, that is, to exist in phenomena other than the one under consideration, (2) to be competent to effect the consequences attributed to it, and (3) to be responsible for these effects.

[3] Incidentally, Darwin's comments in the transmutation notebooks indicate that he already appreciated the social components in the characterization of an explanation as a scientific one: What constitutes an acceptable theory is determined by a scientific community, whose religious and political beliefs are reflected in the criteria. From Herschel, as quoted in Babbage's (1938) "Ninth Bridgewater Treatise," Darwin had concluded that a scientific community whose religious outlook was theistic could accept a putative dynamical theory to account for the origin of species in which the Deity is conceived of as operating through secondary laws. The model of astronomy was the constant referent.

[4] E 95–97; Sir Gavin de Beer (ed.): "Darwin's Notebooks on Transmutation of Species, Part IV: Fourth Notebook (October 1838–10 July 1839", *Bulletin of the British Museum (Natural History), Historical Series*, 1960, **2**: 169–170.

[5] I have interpreted "beings" as individuals. It is also possible in the context of the entry to understand by "beings" not "individuals" but "kinds", a reading which follows from the remark "(all fishes to the state of the Ammocoetus . . .).' Darwin in a famous section of the notebook – "Organized beings represent tree, irregularly branched" – had proposed the splitting of species into branches as a phenomenological fact. B21–23; Sir Gavin de Beer, (ed.), "Darwin's Notebooks on Transmutation of Species, Part I: First Notebook (July 1837–February 1838)". *Bulletin of the British Museum (Natural History), Historical Series*, 1960, **2**: . The reading of "beings" as "kinds" here would suggest that Darwin is postulating a Malthusian multiplication mechanism for species and would constitute an interesting illustration of Darwin's use of analogies in his theoretical models. See in this connection Howard E. Gruber, *Darwin on Man: A Psychological Study of Scientific Creativity, together with Darwin's Early and Unpublished Notebooks*, transcr. and annotated by Paul H. Barrett (New York: E.P. Dutton, 1974, 129–149, and Gruber (1978).

[6] Robert C. Stauffer (ed.): *Charles Darwin's Natural Selection; being the second part*

of his big species book written from 1856 to 1858 (Cambridge/London: Cambridge University Press 1975), p. 234.

[7] Charles Lyell, *Principles of Geology* (London: John Murray, 1830–1833), vol. 2, p. 134. In *Natural Selection* (see n. 6 above), during his discussion of the struggle of existence, Darwin refers to Sir C. Lyell's "equilibrium in the number of species" with the caveat that "it expresses far too much quiescence" (p. 187).

[8] C146; Sir Gavin de Beer (ed.): "Darwin's Notebooks on Transmutation of Species, Part II: Second Notebook (February to July 1838)", *Bulletin of the British Museum (Natural History Historical Series)*, 1960, **2**: 98.

[9] C147e; Sir Gavin de Beer, M.J. Rowlands, & B.M. Skramovsky (eds.): "Darwin's Notebooks on Transmutation of Species, Part VI: Pages Excised by Darwin", *Bulletin of the British Museum (Natural History), Historical Series*, 1967, **3**: 149.

[10] The Charles Darwin Archives in Cambridge University Library, Cambridge, England.

[11] I have elsewhere ("The Origin of the *Origin* Revisited"; see n. 1 supra) referred to other possible sources for such an approach, namely, the philosophical writings of Hume, Smith, and Bentham: that is, the philosophical tradition which based ethics on a pleasure-pain calculus. For this approach in the Scottish circles see Adam Smith; *Essays on Philosophical Subjects*, (ed.) W.P.D. Wightman & J.C. Bryce [with Dugald Stewart: *Account of Adam Smith*, ed. I.S. Ross] (Oxford: Clarendon Press, 1980 – The Glasgow Edition of the Works and Correspondence of Adam Smith, vol. 3), and the essays introducing the texts.

[12] Dugald Stewart, *Lectures on Political Economy*, ed. Sir William Hamilton *The Collected Works of Dugald Stewart*, vols. 8 and 9 (Edinburgh: T. & T. Clark, 1877).

[13] Incidentally, Stewart's Lectures on Political Economy are a rich source for a historical overview of demography, agricultural practices, and agricultural economics during the eighteenth century. See also C.A. Browne's *A Source Book of Agricultural Chemistry* (1944). [full biographical reference needed]

[14] "To begin, then, with that science, which in the judgment of the most enlightened politicians, is the most essential of all to human happiness, – I mean the Science of Agriculture; ow various and important are the subjects which belong exclusively to its province! The general principles of vegetation; the chemical analysis of soils; the theory of manures; the principles which regulate the rotation of crops, and which modify the rotation, according to the diversities of soil and climate; the implements of agriculture, both mechanical and animal; – and a thousand other topics of a similar description" (Stewart, n. 12 supra), vol. 8, p. 11.

[15] Ibid., p. 12.

[16] Ibid., p. 103 ff.

[17] Ibid., p. 107.

[18] Ibid., p. 108.

[19] Ibid.

[20] Ibid., p. 127.

[21] Ibid., p. 128. It is interesting to note that Stewart then suggested that "In general, it should seem, that in proportion as Agriculture advances, the size of farms should be reduced; or rather, that farms should divide themselves in proportion as the task of superintendence became more difficult." The mechanization of farms had not yet begun in 1800!

[22] C. Darwin, *Autobiography*, ed. N. Barlow (London: Collins 1958).
[23] Barbara and Hensleigh Wedgwood *The Wedgwood Circle 1730–1897 Four Generations of a Family and their Friends* (London: Studio Vista 1980).
[24] Sir Gavin de Beer (ed.): "Darwin's Journal", *Bulletin of the British Museum (Natural History*, Historical Series, 1959, **2**: 8.
[25] M108; Gruber and Barrett (n. 5 supra), pp. 286, 302 (n. 84).
[26] M155; Gruber and Barrett, pp. 296, 305 (n. 127).
[27] C267; *Bulletin* (n. 8 supra), vol. 2, p. 114. Not only "lives" but also "works" are mentioned here.
[28] E.g., pp. 17–20; Gruber and Barrett (n. 5 supra), pp. 386–388, 408 (nn. 41, 43, 44, 45); see also pp. 403, 412 (n. 92).
[29] Erasmus Darwin, *Phytologia; or the philosophy of agriculture and gardening* (London: J. Johnson, 1800).
[30] Sir Gavin de Beer (ed.): "Darwin's Notebooks on Transmutation of Species", First Notebook 1837–8; also E. Krause, *Erasmus Darwin* (London: 1879).
[31] *Elements of Agricultural Chemistry*, ed. John Davy, in *The Collected Works of Sir Humphrey Davy*, vols. 7, 8 (London: Smith, Elder, 1840) vol. 8, pp. 80–81.
[32] Ibid., p. 89.
[33] Ibid., pp. 28, 77–79.
[34] His "global philosophy" was outlined in his introductory lecture and deserves to be quoted for his views on the balance of nature: " . . . all the varieties of material substances may be resolved into a comparatively small number of bodies, which, as they are not capable of being decompounded, are considered in the present state of chemical knowledge as elements. . . . The chemical elements acted upon by attractive powers combine in different aggregates. In their simpler combinations, they produce various crystalline substances, distinguished by the regularity of their forms. In more complicated arrangements, they constitute the varieties of vegetable and animal substances, bear the higher character of organization, and are rendered subservient to the purposes of life. And by the influence of heat, light, and electrical powers, there is a constant series of changes; matter assumes new forms, the destruction of one order of being tends to the conservation of another; solution and consolidation, decay and renovation, are connected; and whilst the parts of the system continue in a state of fluctuation and change, the order and harmony of the whole remain unalterable" (ibid., vol. 7, pp. 181–182).
[35] London: Taylor and Walton. Other editions bear varying titles. For a valuable assessment of Liebig and the emergence of agricultural science see M.W. Rossiter: *The Emergence of Agricultural Science: Justus Liebig and the Americans 1840–1880* (New Haven: Yale University Press 1975); J. Liebig: *Chemistry in its application to agriculture and physiology*, ed. L. Playfair (London: Taylor and Walton, 1840); also W. Krohn & W. Schafner: "The Origin and Structure of agricultural Chemistry", pp. 27–52 of G. Lemaine *et al.* (eds.): *Perspectives on the Emergence of Scientific Disciplines* (The Hague, Paris: Mouton 1976). In his *Chemistry* Liebig defined "The GENERAL object of agriculture" as "to produce in the most advantageous manner certain qualities, of a maximum size, in certain parts or organs of particular plants. . . . The rules of a rational system of agriculture should enable us, therefore, to give each plant that which it specially requires for the attainment of the object in view". As his reading lists indicate, Darwin read extensively in the literature of scientific agriculture in the period between

1840 and 1846; see Peter J. Vorzimmer (ed.): "The Darwin Reading Notebooks (1838–1860)", *Journal of the History of Biology*, 1977, **10**: 107–153.

[36] J. Liebig: *Organic Chemistry in its Applications to Agriculture and Physiology* (London: Taylor and Walton, 1840), pp. vi–vii.

[37] J. Liebig: *Animal Chemistry or organic chemistry in its application to physiology and pathology*, ed. W. Gregory (London: Taylor and Walton, 1842) pp. 129–130.

[38] Vorzimmer (n. 35 supra), p. 128.

[39] Vorzimmer "The Darwin Reading Notebooks"; see entry for Nov. 20 1844 and that for 14 Oct. 1851.

[40] Justus Liebig: *Familiar Letters on Chemistry, and its Relation to Commerce, Physiology, and Agriculture*, ed. John Gardner (New York: D. Appleton; Philadelphia: George S. Appleton, 1843), pp. 107–108.

[41] S.S. Schweber, "Darwin and the Political Economists" and "Facteurs idéologiques et intellectuels" (n. 1 supra). [reference to C. Limoges?]

[42] Adam Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations*, ed. Edwin Cannan (New York: Modern Library 1937), p. 423.

CAMILLE LIMOGES

10. MILNE-EDWARDS, DARWIN, DURKHEIM AND THE DIVISION OF LABOUR: A CASE STUDY IN RECIPROCAL CONCEPTUAL EXCHANGES BETWEEN THE SOCIAL AND THE NATURAL SCIENCES*

Much attention has already been paid to the important historical role played in the constitution of scientific discourses by the transference of concepts from one area of knowledge to another. The enquiries generally focus on the analogical or metaphorical nature of these conceptual transfers. Some investigations suggest that these transfers consistently aim not so much at monistic or reductionist explanations as at providing heuristic scaffolding or firmer scientific basis and authority for fledgling domains of knowledge. This is why one should generally expect a typical declivity in the process, the more exact and established sciences providing conceptual frameworks for the less firmly grounded ones. Thence the frequent borrowings of the biological sciences from the physical sciences, and of the social to the biological and the physical sciences.[1]

It is not the contention of this article that such a view is basically wrong. But it intends to exhibit, through the analysis of a sequence of transfers of the concept of "division of labour", that conceptual exchanges between the natural and the social sciences also occured both ways, and that there might be cases where the pay-off seems greater for the natural than for the social sciences.

The concept of division of labour has a long and convoluted history which we cannot even summarize here;[2] it will be enough for our purpose to concentrate the attention on some aspects of three episodes only, those involving the relationships between Henri Milne-Edwards', Charles Darwin's and Emile Durkheim's theorizings.

It has not escaped our attention that the process that we will analyze might seem to form a continuous and neat historical loop: taking root in the tradition of the political economy of Adam Smith, Milne-Edwards' central ideas nourished the thinking of Darwin which in turn provided a crucial building block for Durkheim's sociological theory, a theory which claimed to account for some of the basic assumptions made by Smith. However to posit such a loop would be mistaken.

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 317–343.

Indeed such a schematic understanding of the fate of the concept of division of labour in the nineteenth century would be built upon an artefact of presentation. I do not take here into account other major contributions to the theory of division of labour, such as those of John Ramsey McCulloch, Auguste Comte, Karl Marx, Rudolph Leuckart, Ernst Haeckel, Edmond Perrier and many others, because they remain at best marginal in relation to the development of the thinking of our three authors, and also because they are not needed for the issue I want to investigate. Moreover such closing of a circle would give the impression of a theoretical continuity which in fact did not exist between the episodes under analysis: the transferences of concepts between scientific domains ought not to be necessarily equated with simple borrowings or isomorphic processes.

It is in this perspective that the presentation of each of the three cases will proceed first with a succinct description of the notional transfer, briefly clarifying its function and genealogy, and then with an attempt at elucidating the exact nature and significance of the processes involved.

1. FROM POLITICAL ECONOMY TO BIOLOGY

Henri Milne-Edwards and the Concept of a "Division of Physiological Labour"

The concept of division of labour appeared in the biological literature, almost simultaneously, in the works of Peter Mark Roget and of Henri Milne-Edwards.[3] However while Roget's fame rests essentially on his much celebrated *Thesaurus* (it took a century and a half before his possible priority in the use of the notion of division of labor for biological purpose was noticed[4]), "division of physiological labour" became Milne-Edwards' trade mark. Indeed he became the leader of the "School of physiological zoology" which exercised in mid-nineteenth century France an hegemony equaled before only by Cuvier.[5]

Milne-Edwards made no mention of Roget, and it is unlikely that he could have borrowed from him the idea to apply the notion of a division of labour to the understanding of the structure and functions of animal organisation.[6] However, it is clear that Roget also derived his understanding of division of labour in the organism on the basis of the political economists' views of the rationale for the increasing division of labour in the manufactures and the factories and of its beneficial consequences:[7]

in any case it remains that the concept of a division of physiological labour did derive from political economy.

Whereas Roget used almost cursorily the concept of division of labour, and essentially as a metaphor to describe the chemical function of nutrition in the different classes of animals,[8] did not develop it any further and made of it no theoretical use, it came to occupy in Milne-Edwards' works a central theoretical position and played a crucial role in his attempt to supersede Cuvier in providing a new basis for the practice of natural classification. His work attracted much attention in his time, notably from Darwin. All this makes it necessary that we focus here on Milne-Edwards and not on Roget.

As Milne-Edwards pointed out in his 1827 articles, lower organisms are so named because their diverse physiological functions are assumed by organs less differentiated, less specialized than in the so-called higher organisms. In the simplest of them, such as the fresh-water polyps, the structure is homogeneous, so that all their parts concur in the same way to the maintenance of life. Indeed, this is what Abraham Trembley's famous experiments on the complete regeneration of whole individuals from any segment of a polyp cut into minute pieces had demonstrated. This, concluded Milne-Edwards, shows that "The body of these Animals may be compared to a workshop [*atelier*] where all workers are employed at similar tasks and where, therefore, their number affects the amount, but not the nature of the result." In higher organisms, where "life manifests more complex phenomena", organs become specialized, there is a "localization of functions" and the life of the individual is no more the sum of the workings of elements of the same nature; it becomes the result of a set of essentially different actions produced by distinct organs. "The diverse parts of the animal economy cooperate toward the same objective, but each one in its own manner; and the more numerous and developed the faculties of a given being, the more elaborate are the diversity of structure and the division of labour".[9] "In a word, it is always according to the principle of division of labour that nature perfects the results it wants".[10]

In the following years, Milne-Edwards developed these views at greater length in an environment particularly congenial to associations between natural history and political economy, the *Ecole centrale des Arts et Manufactures*, where he started to teach in 1831. There again, in his teaching, he emphasized that each function in a living body is a labour process [*travail*], and that the principle of division of labour guiding nature is "precisely one of those which have been the most influential on the progresses of human industry".[11]

But it is in pursuing his series of studies on Crustaceans (started with Victor Audouin in 1826), in which he investigated by dissection and experimentation the diverse anatomical arrangements by which the physiological fonctions are performed, that he gave his new theory its full extension and showed how it provided a basis for a new approach to the practice of natural classification. It is beyond the scope of this article to examine Milne-Edwards' work as a zoologist and taxonomist, but it is important to emphasize that division of labour was not just a superficial metaphor, not only a fashionable catch phrase to attract attention or some useless abstract superstructure, but functioned as a central concept for his practice as a scientist. As he himself stated in his first major work, the *Histoire naturelle des Crustacés*,[12] the principle of the progressive division of physiological labour provided the rationale for substituting a new method to the Cuvieran approach to taxonomy. The use of the Cuvierian principle of subordination of characters led to group together only those organisms which showed similar internal anatomical structure, whereas adopting the new view one would not be side-tracked by differences in structures and would "include in the Crustaceans all the animals whose general organisation, even when less complex, is bounded with the organisation of the types of the class, and whose conformation recalls the transitory states through which went the most perfect beings of the series during their embryonic life". By so doing one could, under the conditions stated above, bring in the same class animals with gills and others which have no special organs for respiration and where this function is performed through the skin, animals which have a heart and a complex vesicular system and others which have no distinct vessels, animals with highly specialized organs with others which show for the same function only rudimentary apparatuses. This is why some degenerated [*dégradés*] animals put with zoophytes by Cuvier, were classified by Milne-Edwards as Crustaceans.[13] Despite his avowed non-evolutionary standpoint, the outlook of his taxonomy, emphasizing the "tendency to the perfecting of the organism by the division of physiological labour",[14] in many regards is congruent with what one would expect from an evolutionary classification, and it is no surprise that Darwin, as we will see, made thorough analysis of many of Milne-Edwards' contributions.

Milne-Edwards gave his most elaborate statement on the nature and significance of division of physiological labour in 1851, in his *Introduction à la zoologie générale*.[15] The theoretical intention of the work is explicit: it aims at unravelling the general tendencies of nature in the constitution of the animal kingdom and its astonishing diversity of forms. Milne-Edwards basic hypothesis is that nature must have proceeded the way we would, according to a "law of economy" permitting the generation of a broad diversity of products with the minimum of innovations.[16] This is how there are few great types of animals, each one exhibiting a single general anatomical plan and, at the same time, thousands of diverse species of organisms.

Among the causes for that diversity, is the "tendency to vary the degrees of perfection" of the organisms. This is done either by increasing the outputs, or by improving the quality of the products.[17]

According to Milne-Edwards, increased production is first obtained by increasing the mass of living tissues responsible for the performance of vital actions;[18] so that the zoological rank of organisms is, in a general way, related to their mass.[19] The most economical way to obtain an increased number of organs is for nature to repeat them. This is why, specially in inferior taxa, such as Annelids or Echinoderms, identical segments are aggregated.[20]

However the rank of organisms is less dependent upon the simple sum of the "vital force" they exhibit than on the precision and diversity of their physiological actions. This cannot be derived from a simple increase in the number of similar organs through repetition. "In the creations of nature, as well as in human industry, it is mainly through the *division of labour* that improvements are obtained."

Any living being is similar to a workshop in which organs are comparable to workers toiling at the production of the set of phenomena which constitute its life as an individual. In segmented animals, where the parts present the same physiological properties, the final result is coarse and of little value; the individual shows an aggregation rather than an association of producers and the organism is like a badly managed workshop where each worker performs the complete series of operations, so that their number influences the quantity but not the quality of the products. On the contrary, where division of labour occurs and where the organs become more and more specialized in their "vital labour", their results ultimately present an "exquisite perfection".[21]

With the increasing diversity of roles in the organism, the independence of its parts is progressively reduced. The harmonious functioning of the whole requires more and more of a strict coordination and the subordination of each of the physiological agents of the association.[22] This is not achieved either through the influence that the parts would mutually exercise upon one another, or through the influence of a "dominating" character or part, as Cuvier believed. It must rather result from the action of a preexisting force, plan or power, the nature of which could be elucidated only through the investigation of the origin of the living beings, of heredity and of the permanence or variability of species.[23]

But this is the concluding statement of the book and Milne-Edwards personal views on the origin of species at this stage remain unknown;[24] as we mentioned earlier, the second part of the *Introduction à la zoologie générale* was never published and there does not seem to be any evidence that he ever started writing it.

An Analogical Use of Concepts and Its Limitations

As we have seen Milne-Edwards himself repeatedly emphasized that the notion of a physiological division of labour holds its intelligibility from political economy.

Though the borrowing from political economy is unquestionable, the precise source of his knowledge of the economics of his time remains unclear, since he seems to have nowhere quoted or mentioned by name any economist in his writings. One may surmise however that coming from a wealthy English business family background,[25] basic notions of that discipline, as popularized in the culture of his milieu, were early assimilated.

Division of labour, according to Adam Smith "increases the productive powers of labour", it "increases the dexterity of the workers", saves time, increases the quantity of work an individual is able to perform, and favors the progressive invention of new means to facilitate and abridge work.[26] According to Milne-Edwards, the benefits of the division of physiological labour roughly parallel those of the process applied in modern manufactures: it increases the "vital powers"[27] of the organism, improves the "quantity [*grandeur*] of the results"[28] and the "quality of the products" of "vital work";[29] moreover, once started, the tendency of nature is to "increase the number of dissimilar parts and the complication of the machine".[30]

One might wonder how it is that it took fifty years, after 1776, before anyone thought of transferring the concept of division of labour from political economy into biological theory. However unless one believes in the quasi-animistic power of ideas to impose themselves upon a domain of knowledge for the simple reason that they have already appeared elsewhere, there is no reason to assume the necessity of such a transfer. After all, most scientific concepts are not migrants. Again, and for similar reasons, it is no more convincing to assert that it is the socio-economic reality of the time which necessitated the emergence of the concept of division of labour as well in political economy as in biology. It could not either account for the half-century time-lag. Moreover, the first theoretician of division of labour was not Adam Smith but Bernard Mandeville who himself wrote more than fifty years before the publication of *The Wealth of Nations*, that is, long before the industrial revolution took shape.

If the transfer did occur in Milne-Edwards' work, it is rather because the concept of a division of labour seemed to him to offer a solution to a problem. Such a transfer could not occur before it was seen as useful; where there is no problem there is no occasion to look for an answer and the notion of an answer in the expectation of its problem is historical nonsense. This is why questioning the reason for the long delay after Adam Smith does not appear specially meaningful. However understanding why this is not meaningful, calls our attention to Milne-Edwards' problem.

Cuvier had assumed as a basic principle the primacy of function over structure. He was however through and through a comparative anatomist and maintained that the functional-anatomical correlation was so tight that subordination of characters could be understood as applying to the organism as to a machine. As he wrote in the dedicatory letter to his *Leçons d'Anatomie comparée*: "The machines which are the object of our researches cannot be taken apart without being destroyed", implying that physiological researches were of limited utility. On the contrary, "a single fact of comparative anatomy has often been sufficient to destroy a whole edifice of physiological hypothesis".[31] In practice, not unlike the older tradition of considering physiology as *anatomia animata*, Cuvier was satisfied in positing a function where the apparatus to perform it existed.

This is what Duméril implied in the report he wrote under Cuvier's name and his own on Audouin and Milne-Edwards' "Recherches anatomiques et physiologiques sur la circulation chez les Crustacés" presented to the Academy of Sciences in January of 1827. This paper described some experiments made to establish that, despite the peculiarities of their anatomy, Crustaceans have a circulation similar to that of the Molluscs.[32] The report of the two academicians reviewed favourably the contribution of the younger scientists who had "fully confirmed an opinion expressed by Mr Cuvier in his *Leçons d'anatomie comparée*", but Duméril could not refrain from stating that, though interesting, the physiological experiments were not necessary since "the result could be deducted and well conceived only after the anatomical researches".[33]

However Milne-Edwards could not agree with this last statement, and more and more his approach was to diverge from that of Cuvierians of strict obedience. By working mainly on marine organisms, he had become convinced that functions may exist without the complex apparatuses found in higher animals and that Cuvier's theoretical framework of subordination of characters, correlation of parts and dominating characters were much too anatomically biased. This did not mean however that organisms were haphazardly constituted; some principle had to account in an orderly fashion for the looser but real relationships between function and structure. The principle which filled this need for intelligibility was the division of physiological labour, which emerged precisely in 1827.

The problem had found its solution. In a way the concept of a division of physiological labour permitted Milne-Edwards to adhere even more strictly than Cuvier to the principle of the primacy of the function; it did not lead to a definitive rejection of Cuvierian concepts such as subordination of characters and dominating characters, but rather to a less mechanistic and more physiological interpretation of their meanings.[34] As his contemporaries quickly perceived, and Darwin among them, Milne-Edwards' approach was a fruitful one for classification and there is no doubt that, not only in France, the considerable success of the "school of physiological zoology" was facilitated by the fact that its leader appeared less as an opponent to Cuvierism than as a revisionist Cuvierian.[35]

Nevertheless in retrospect one can see that Milne-Edwards' theory was not without its problems. These hinged around two issues: what is the use of division of labour? what is the causal dynamics of this process?

As for Adam Smith, for Milne-Edwards the purpose of the division of labour is the increase in productivity, in the quantity and quality of the results, in "the value of the sum of products".[36] There is however a significant difference between the two authors: whereas Smith's theory accounts for an objective evaluation of the success and optimal limits of division of labour in a given environment at a given time, this is not the case in Milne-Edwards'. In Smithian political economy, the properties of the market, on which prices are set, provide the criteria for an assessment of the division of labour.[37] It is not so in Milne-Edwards. In transfering the notion of a division of labour into biology, he did not import with it these major contextual elements.

Such an importation would have implied a reevaluation of the current proto-ecological tenets on the "economy of nature". However like all his contemporaries in France, Milne-Edwards still subscribed to the classical Linnean concept of an economy of nature,[38] which entailed that the living universe was well regulated, that an equilibrium was maintained between all populations of living species playing a preordained role in the economy of the whole.[39] Because of that assumption of stability, the economy of nature could not be conceived as an evolving market and no connection could be made between environmental conditions and the progress and extension of the division of physiological labour. Under these conditions, that process could be assessed only in subjective and ultimately aesthetic terms. This is why the concept of "perfection" plays such a crucial role in Milne-Edwards' theory.[40]

Indeed the notion of perfection occurs in most pages of the *Introduction à la zoologie générale*. As we have seen, Milne-Edwards started from the observation that nature tends to "vary the degrees of perfection" of the organisms.[41] He also distinguished between the "power of action", which can be increased through repetition of parts, as in segmentary organisms, and "real perfection",[42] a perfection of the physiological functions, which is forwarded by the division of physiological labour.[43] That increased perfection is made possible by the progressive localisation of the functions in special apparatusses; this is, very helpfully for the taxonomists, reflected in anatomical complexity, however there is no direct measure of the increased "yield" of the perfected functions.[44] All that can be said is either that a biological process similar to an economic one should produce similar effects, i.e. increase efficiency,[45] or that no one will deny that with increased division of labour the more "exquisite" the faculties become.[46] Since, as we have seen, Milne-Edwards could not depend entirely on anatomical criteria, and since the economy of nature could not function as a Smithian market, he could only rely upon analogy and quasi-aesthetic criteria. Given his basic assumptions, there was no way out and the theory was to keep that form in his later works.[47]

If the use and efficiency of the division of physiological labour were difficult to assess objectively, the very cause driving the process, as we mentioned earlier, "the power which acts in a different way upon each species"[48] remained mysterious. Milne-Edwards had to be content with refering to the two regulating principles of nature: "the need for variety" and "the tendency to economy".[49]

In summary, as Milne-Edwards made clear from the start,[50] it is a new theory that he intended to develop, and division of labour was the key concept of his theoretical construction. He applied the notion to a new object, the physiological functions, but without its conceptual context, prices, the market and the circulation of goods. The transfer permitted him to assume by analogy effects in the organism similar to those in the economy, as far as production only was concerned. Because the theoretical relationships in which was embedded the Smithian concept of division of labour were not reconsidered and reworked in the field of application of the transfer, the theory of the division of physiological labour, despite its empirical fruitfulness in classificatory practices, ultimately proved to be of dubious theoretical consistency. The contrast with Darwin is enlightening.

2. A TRANSLATION PROCESS INTERNAL TO BIOLOGY

Charles Darwin and Evolutionary Divergence As Division of Ecological Labor

Charles Darwin carefully read and annotated many of the works of Milne-Edwards. As early as 1839, Darwin mentioned in his *Notebooks on Transmutation of Species* Milne-Edwards' "admirable paper on the geographical distribution of Crustacea".[51] In December 1846, having read the "Considérations sur quelques principes relatifs à la classification naturelle des animaux, et plus particulièrement sur la distribution méthodique des Mammifères",[52] while reading in parallel Milne-Edwards' *Histoire naturelle des Crustacés*,[53] he wrote:

This is the most profound paper I have ever seen on Affinities. It is quite curious how such words as 'zoological parentage' 'descent sous differents types' are used metaphorically. In future ages such language will be a wonder – p. 66 shows embryology best means for classification – gives examples in Custacea Isopod (see his Treatise on Crust 1st vol. for excellent remarks) the specific characters are not assumed till after the generic & these after those of the family [. . .] p. 72 compares metamorphosis of embryo in the animal kingdom to bundle, which individuals at different heights. p. 76 compares the classification as deduce from embryo to a tree – remark the animals ought to arrange in *space* and not in a *plane* surface p. 76 the character of superiority in any series is 'l'empreinte plus profonde du cachet propre à cette même série, et l'adaptation plus complète du plan organique ainsi constitué à la division du travail physiologique' says that the most radiate animal with the greatest number of distinct organs is higher than the binary form [. . .] p. 80 Remarks that in two serial groups that the resemblance is greatest between the lower forms [. . .] p. 89 Fish & Batracians, continue for long to walk in the same embryonic road whereas Mammals, Birds, Reptiles, at an earlier date begin *to diverge*[54] or: 'la marche génésique parallèle (in these) est de moins longue durée'.[55]

This is evidence not only that Darwin highly esteemed and carefully read Milne-Edwards, but, above all, that from the mid-1840's on, he found in the latter's writings inspiration in his meditations on the progressive branching of taxa. On these readings, it could not – and did not – escape his attention that "divergence" (a word Milne-Edwards also often used) was related to the increasing division of physiological labour from the lower to the higher animals. However, as we have seen, Milne-Edwards himself was at a loss to explain by what causal agent divergence through division of physiological labour might have actually occured. Though he did make Darwin accustomed to associate the two concepts, he did not provide a solution that could simply be borrowed to causally explain divergence in evolutionary terms.

Divergence was no minor issue for Darwin. Indeed, he came to see it as a "keystone" of his theory,[56] as the regulating principle of the natural selection of variations,[57] and Ernst Mayr has noted that he always referred to it "with great excitement".[58] In a nutshell, Darwin's solution was that the more diversified the organisms become through variations, the easiest it will be for them to occupy specialized niches in the economy of nature, and to escape elimination through competition, to survive and to reproduce.[59]

In *Natural Selection*, the unfinished longer version of what was to become his major work, Darwin stated concerning the "principle of divergence": "This doctrine is in fact that of 'the division of labour', so admirably propounded by Milne Edwards . . . ";[60] and in the *Origin of Species*, he again, in his presentation of the "principle", referred to Milne-Edwards and the division of physiological labour.[61] This is why, in 1968, I pointed out that divergence could be called Darwin's principle of the "division of ecological labour", and suggested genealogical relationships between the views of the two biologists.[62]

In his *Autobiography* Darwin depicted the solution to the problem of divergence as an insight which suddenly occured to him,[63] and he elsewhere gave hints that seem to permit to date this event *circa* 1852.[64] This seems to coincide with the time he read Milne-Edwards' *Introduction à la zoologie générale*.[65] However Dov Ospovat believed that most of Darwin's solution was worked out between November 1854 and January 1855 and that the solution was given its final form around September 1856,[66] whereas David Kohn has asserted that the concept of division of labour played no role in the emergence of the solution.[67] There are good reasons to disagree with this last view.

At the very moment when according to Kohn the structure of Darwin's argument was "dashed off", in November 1854, Darwin wrote a most significant note:

> There is no law of Progression, but time would give better chance of sports, and allow more selection; and all the organisms then living an advantage, – a free competition of labour – the result would be complicated and more perfect.[68]

The weird phrase "competition of labour" is particularly significant here in its conflation of the two notions of competition and division of labour, the combination of which is central of Darwin's solution. As this note would indicate, at least from his reading of Milne-Edwards' *Introduction* in late 1852, division of labour was a concept to "work by" as far as the enigmas of classification and divergence were concerned.

Again, this is not to say however that from a theoretical standpoint Darwin borrowed his solution "stock and barrel" and that Milne-Edwards had already made a similar use of the concept. Division of physiological labor had undergone a substantial transformation before it could emerge under the guise of divergence understood as division of ecological labour.

Transforming a Concept for Theoretical Purposes

In Darwin's work division of labour and its associated concept of divergence play a central theoretical role, indeed it is the keystone of the complex architecture of the *Origin of Species*. As we have seen, division of labour there applies to a geographical area where competition favours those organisms which present variations procuring adaptive advantages in the occupation of new ecological niches, "places in the economy of nature" in Darwinian terminology. Under these conditions those organisms will have better chances to survive and reproduce. If variants that diverge the most from the mean of the population benefit from differential reproduction, the incipient species will naturally tend to diverge more and more from one another, which accounts for the ramified, tree-like structure of phyla. Divergence seems to be a function of the partitioning of the economy of a given area into more numerous and specialized places or niches, of making the ecology of the area a more complex structure. It is the process which Darwin likens with Milne-Edwards' division of physiological labour:

The advantage of diversification in the inhabitants of the same region is, in fact, the same as that of the physiological division of labour in the organs of the same individual body – a subject so well elucidated by Milne Edwards. No physiologist doubts that a stomach by being adapted to digest vegetable matter alone, of flesh alone, draws more nutriment from these substances. So, in the general economy of any land, the more widely and perfectly the animals and plants are diversified for different habits of life, so will a greater number of individuals be capable of there supporting themselves.[69]

However we clearly have here a process of conceptual transfer much different from the one previously encountered in Milne-Edwards' works.

It is true that Darwin too used the notion of division of labour to stress an analogy of results between two processes, as they occur in different circumstances, in organisms or in the competitive relationships between organisms. This is what Milne-Edwards himself had done bringing together economic production and physiological functioning. It is also true that despite the fact that he made use of a number of Milne-Edwards' examples and expressions, he no more than the latter had done with the political economists imported the explanatory structure found in the work of the French zoologist; Darwin even explicitly, though privately, denied any foundation to his predecessor's understanding of the principles at work in nature, characterizing them as "metaphorical rubbish".[70] However, this is precisely the point where they differ: whereas Milne-Edwards ignored the conceptual context of the Smithian concept of division of labour, Darwin was firmly critical of the quasi-transcendental theoretical construction of the Frenchman and that, because of his own commitment to causal explanation in biology.

Milne-Edwards was no economist and his interests were those of a biologist; he could ignore the context of the concept he borrowed. Not so with Darwin: division of physiological labour was a biological concept and he could not, as a biologist, ignore what it entailed from a biological vantage point. For Darwin, the division of physiological labour, as described by Milne-Edwards, is indeed a useful tool to understand the differences between lower and higher organisms as their functions become more specialized and localized, supported by anatomical apparatuses which correspond to the increasing complexity of organisms; however the division of labour has to be explained and understood by the complex relationships that the organisms entertain with one another in the struggle for existence: ultimately it is the division of ecological labour, that is divergence, which accounts for the division of physiological labour. What we have here is not merely the transfer of a concept but a reworking of its fundamental significance, and the production of a new concept at the ecological level.

Darwin's concept of a division of ecological labour is not really homologous to that of the division of physiological labour.[71] Whereas the latter works to the advantage of the whole organism – it would make no sense to say that it works for the organs –, in the case of divergence where the set of "places in the economy of nature" is the analogue of the organism, the advantage is for the organs, that is the individual organism or the species.

No doubt, a conceptual transfer there was, but what was transferred underwent in the process a deep transformation: the concept borrowed, that of the division of physiological labour found at last its explanation on the basis of the seemingly twin concept of the division of ecological labour, and this last concept was in fact differently structured and given a totally different denotation.

3. FROM BIOLOGY TO SOCIOLOGY

Emile Durkheim and the Concept of a Division of Social Labor

The first theoreticians of division of labour, Bernard Mandeville, Adam Ferguson or Adam Smith were concerned with society, morality, and the economy. One might have expected that Emile Durkheim,[72] who wanted to establish sociology as a science, would have started where they left. He certainly was not unaware of that tradition,[73] but it is in the biology of his time that he found some of the crucial conceptual elements for his own theorizing.

De la division du travail social, his doctoral dissertation, was published in 1893. It was his first major work and became a classic of the sociological literature.

Durkheim's basic problem was to solve the apparent antinomy between the increasing autonomy of the individual in history and its increasing dependence upon society. The solution, he found in the transformation of social solidarity depending upon the progressive development of the division of labour.[74]

According to Durkheim, the division of labour is not an institution generated mainly by man's intelligence and will.[75] Biologists have shown that it is a much more general reality than the economists have believed and that its law applies as well to organisms as to societies.[76] The division of social labour is a particular form of this general biological process and the societies in conforming to this law "seem to yield to a current born long before them, which carries in the same direction the entire living world".[77]

As we have shown, Milne-Edwards distinguished between two types of organic complexity: the one obtained through repetition of similar parts in segmented animals and the other generated through division of physiological labour. In a similar manner Durkheim also distinguished two types of societies: the segmental societies and those characterized by division of social labour.

The segmental societies are those "formed through repetition of aggregates similar one to the other, [such as clans], analogous to the segments [*anneaux*] of the annelid".[78] Originally, these societies must have derived from a sort of "social protoplasm",[79] from undifferentiated, homogeneous social masses, which one has to postulate, though no society has actually been described which would exactly correspond to that state. In segmental societies, constituted by concatenation of undifferentiated groups, the individuals are no more than "social molecules",[80] characterized by "mechanical solidarity", a sort of mental, religious and material communism and conformism where individuals are socially interchangeable, a collective consciousness where individuality cannot emerge in its singularity.[81]

In contrast, the societies where the division of labour predominates are characterized by an "organic solidarity" based upon an increasing mutual dependance of the individuals. As these individuals specialize more and more in the performance of diverse social functions, they also become more and more strongly characterized by individual differences. In these societies, only traces of mechanical solidarity or collective consciousness subsist. Where organic solidarity obtains, the social elements are not any more placed side by side, or encased into one another, but coordinated and subordinated the one to the others around a central organ exercising a moderating action on the whole.[82]

This typology entails a crucial question for the Durkheimian understanding of historical dynamics: indeed, one has to figure out how it is that segmental societies gave rise to societies characterized by organic solidarity.

Durkheim makes clear that he cannot accept Herbert Spencer's hypothesis of a development, of a progressive manifestation of individuality through historical times, made possible by the removal of the causes of its oppression and occultation. By definition there is no individuality in segmental societies. For the same reason, he could not be satisfied with Adam Smith's psychological explanation of the emergence of division of labour through the development of barter and exchange between individuals eager to increase their pleasure, well-being and happiness.[83] For Durkheim, there would be contradiction to assume that the social molecules behave as individuals. Clearly the properties of segmental societies were obstacles to the emergence of societies characterized by division of labour and organic solidarity. How then could they have given rise to them? This was no minor problem since the logical consistency of the Durkheimian theory depended upon its resolution.

According to Durkheim, the transition from one type of societies to the other is made possible by the increase in population, provided it goes with an increase in the condensation or social density of that population. Then the segmented structure is loosened, the segments loose their individuality, their partitions become permeable, they coalesce and free the "social matter" which can enter into new combinations.[84] The causal explanation here is explicitly Darwinian:

> If labour is divided in proportion that the societies become more voluminous and dense, it is not because of more varied external circumstances, it is that the struggle for life becomes more fiery.
>
> Darwin has very properly observed that the competition between organisms is all the more sharp that they are more analogous. With the same needs and going after the same objects, they are everywhere rivals. As long as there is more resources than they need, they can live side by side; however if their number increases in such proportion that all their appetites could not be any more entirely satisfied, war breaks out, and it is all the more violent that the shortage is pronounced, that is to say that the competitors are numerous. It is altogether different if the coexisting individuals are from different species or varieties. [. . .]

Men submit to the same law. [. . .] it is easy to understand that any condensation of the social mass, specially if it goes with an increase in population, necessarily determine the progresses of the division of labour.[85] [. . .] The division of labour is a result of the struggle for life: but with a softer outcome.[86]

It is this progressive disjunction which Darwin has called the law of the divergence of characters.[87]

By applying a biological concept, Darwin's principle of divergence or of a division of ecological labour, Durkheim seemed to have provided to his problem a solution consistent with his typology: division of labour emerged from segmental society not by the will of individuals, but according to a blind process at work in the interactions of all living beings.[88]

Unproblematized Conceptual Transfer As Illusory Solution

Steven Lukes has pointed out the highly metaphorical style of Durkheim and the fact that it is the organic analogy which predominated in the *Division du travail social.* This style, he stressed "tended to betray Durkheim into misrepresenting his own ideas, and into misleading himself and his readers as to their significance".[89]

According to Lukes, on the point which interests us here, Durkheim's theory is a sociological explanation of the growth of differentiation, and not a biological one as many interpreters, like Sorokin or Parsons, have asserted. This misrepresentation would come from the wrong assimilation or reduction of demographic phenomena to biological ones, whereas Durkheim's argument in any case is not that the division of social labour is explained only by population growth, but by the more active intercourse between individuals resulting from "dynamic or moral density", a sociological process which Auguste Comte has identified before.[90] This is, I think, a well taken point.

However, one should note, as Lukes mentions himself, that it is only a couple of years later, in the *Règles de la méthode sociologique*, that Durkheim recognized that "material density" is not an "exact expression of the dynamic density".[91] In that respect the *Division du travail social* remained equivocal. But this is a secondary question. The real issue concerns the mechanism by which the progress of the division of social labour occurred; Durkheim's point here is not that it is explained by simple population growth or increased "moral density", but that, when these conditions obtain, it is the struggle for existence, the Darwinian principle of divergence which accounts for the incipient and all further stages of the social division of labour. As we have made clear earlier, it is the biological concept of a division of ecological labour which is here squarely applied to the social process; it is not metaphorized in any way but, on the contrary, is borrowed and used for its explanatory power because of what is taken as its facticity.[92]

It has often been noted in the sociological literature that in none of Durkheim's later works, or those of his school, is the emphasis on the distinction between mechanical and organic solidarities made any use of. So that, what was in 1893 the crucial query of social morphology, and its biological solution, lost all relevance. This may be because Durkheim came to see the attributes of mechanical solidarity, of collective consciousness as the "eternal characteristics of social facts in general", as Robert Nisbet has suggested;[93] it may also be that he became dissatisfied with a theoretical construction bearing too heavily on biology while he stressed more and more the autonomy of sociology as a discipline.

Indeed, as quotations we have given earlier show beyond doubt, at the time he was writing the *Division*, Durkheim still believed that some social processes, like the division of labour, conform to deeper processes, to a "current born long before [societies] which carries in the same direction the whole living world".[94] This should be taken as a statement of fact and not as an analogy. At the time, Durkheim was still very much of a Comtean positivist, and it made sense for his intellectual project to found sociology on the basis of biology, as was required in the positivist classification of the sciences.

It remains, as Lukes has also emphasized, that Durkheim's "central, morphological explanation of structural differentiation is incomplete and largely speculative, saying very little about exactly how competition is resolved and virtually ignoring (unlike Spencer's theory [or Darwin's for that matter]) the vital permissive influence of features of the physical environment."[95] Durkheim's attempt at superimposing a conceptual framework, that of divergence, upon an order of reality different from the one it was constructed for, proved to be a failure, or at best an "extraordinary fable".[96]

4. CONCLUDING REMARKS

As we noted earlier, with division of labour, what may seem at first a straightforward series consisting in three transfers (from political economy to biology, from the work of one biologist to another's and from biology to sociology), constitutes in fact a rather complex chain of substantially different operations. To speak of transfer as a univocal process would here be deceitful.

In the first case, it is an idea which is borrowed rather than a concept with its set of theoretical relationships, and that idea is put to work analogically into a new and totally different theoretical context. The new concept it helps to construct, that of a division of physiological labour, plays in Milne-Edwards' work a central role; it helps to bring together a great wealth of anatomical and functional data. However from a theoretical vantage point it is a *mimetic concept* which goes no further than the analogy itself: its actual biological foundation remains unexplained. In fact, this mimetic concept is theoretically poorer than the economic concept of a division labour inserted in a tight structure including the market, competition and prices.

In the second case, the concept as analogically applied by Milne-Edwards is transfered with the biological relationships it entails; however this concept is explicitly seen by Darwin as a metaphor and thence as requiring a truly theoretical foundation. Darwin makes repeated uses of the concept of the division of physiological labour as such, but provides an explanation for the very existence and progressive perpetuation of that process through another process, that of the division of ecological labour, the principle of divergence. On the other hand, with Darwin, the division of physiological labour, as we have seen, is not only a pedagogical device – which it also is – but also a heuristic element in the construction process of a new concept. That concept of divergence, or of a division of ecological labour, however is not restricted to the analogical reproduction of the original concept; it is the object of a complete reconstruction and incorporation into a complex new theoretical structure involving other concepts such as adaptation, variation, struggle for existence, natural selection, places in the economy of nature. What is significant here is not the transfer *per se* but comes from the reworking of the concept.

Durkheim's case, again, is substantially different. It is not as an analogy that divergence contributes to the understanding of the dynamics of the emergence and progression of the division of social labour: indeed, it is the very Darwinian process of the division of ecological labour which is presumed to occur and to produce the passage from segmental societies to those characterized by organic solidarity. Here we do have, in the strictest sense, an attempt at the full transfer of a conceptual explanation.

Conceptual transfers may have very different functions, not necessarily mutually exclusive. They may serve as heuristic and theoretical construction tools, as pedagogical and persuasion devices, as polemical weapons[97], as legitimizing labels, or as evidential support.[98]

When they concern the core enterprise of science, theory construction, the function of conceptual transfers is to bridge the gap between the known and the unknown. This always involves the risk to forget that analogy is a "mere prolegomenon" and that it is not enough to substitute the known, elsewhere constructed, to what remains unknown and thence to be actively conceptualized in specific terms in the new realm of problems.[99]

As the editor of this volume has emphasized in his book on the *Newtonian Revolution*, any significant scientific achievement is always more than a simple aggregation or synthesis of existing concepts. These concepts are not merely transfered, they are questioned, reworked and transformed.[100]

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NOTES

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1 See for instance: Owsei Temkin: "Metaphors of Human Biology", pp. 169–194 of Robert C. Stauffer (ed.): *Science and Civilization* (Madison: University of Wisconsin Press, 1949); Georges Canguilhem: "Le problème des régulations dans l'organisme et dans la société", *Cahiers de l'Alliance Israélite universelle* (Paris), 1955 n° 92: 64–81; Georges Canguilhem: "Modèles et analogies dans la découverte en biologie" in *Etudes d'histoire et de philosophie des sciences* (Paris: Librairie philosophique J. Vrin, 1968), pp. 305–318; Judith E. Schlanger: *Les métaphores de l'organisme* (Paris: Librairie philosophique J. Vrin, 1971); Michael Ruse: "The Value of Analogical Models in Science", *Dialogue*, 1973, **12**: 246–253. Also, the remarkable analysis of Claude Blanckaert: "Variations sur le darwinisme, epistémologie et transfert lexical", pp. 9–47 of Martine Groult, Pierre Louis & Jacques Roger: *Transfert de vocabulaire dans les sciences* (Paris: Editions du Centre National de la Recherche Scientifique, 1988).

2 The critical history of the concepts and theories of division of labor is the subject of a forthcoming book.

[3] Peter M. Roget: *An Introductory Lecture on Human and Comparative Physiology* (London, 1826); Henri Milne-Edwards: "Nerfs", pp. 529–534 of *Dictionnaire classique d'histoire naturelle*, vol. 11, (published in January 1827); and "Organisation", *ibid.*, vol. 12, pp. 332–345 (published in August 1827).

[4] See Bernard Balan: "Premières recherches sur l'origine et la formation du concept d'économie animale", *Revue d'histoire des sciences*, 1975, **28**: 289–326; see pp. 290–291.

[5] For a summary of Milne-Edwards' life and career, see J. Anthony: "Henri-Milne Edwards" in *Dictionary of Scientific Biography* (New York: Charles Scribner's Sons), vol. 9; Also: A de Quatrefages, E. Blanchard, M. Frémy, de Lacaze-Duthiers, & L. Passy: *Discours prononcés aux obsèques de M.H.-Milne Edwards, le vendredi 31 juillet 1885* (Paris: Institut de France, 1885); E. Perrier: "Henri et Alphonse Milne-Edwards", *Nouvelles Archives du Muséum d'Histoire Naturelle*, 4th series, **11**, pp. xxix–xlviii; de Lacaze-Duthiers: [Henri Milne-Edwards], *Revue scientifique*, 1885, pp. 166–169. Anon.: "Sketch of Henri Milne-Edwards", *Popular Science Monthly*, 1883, **22**: 545–549.

[6] The first mention of "*division du travail*" by Milne-Edwards occurs on p. 534 of his article "Nerfs" (n. 3 supra) published in January of 1827; this article must have been written at least many weeks before Roget's book was published in very late 1826, or maybe even in 1827, despite the inscription of 1826 on the title page (a not uncommon discrepancy), since his "Advertissement" is dated "7 November 1826". This book reproduces lectures given in October 1826.

[7] See (n. 3 supra), pp. 61–62: "In the lowest orders of the animal creation, these functions [digestion and assimilation of nutriments] are conducted in the simplest manner, and by the smallest number of organs. We may compare the reparatory system, in this case, to a manufactory on a frugal base, conducted by ruder methods, with a scanty apparatus, and by the smallest possible number of workmen. In proportion as we ascend in the scale of animals, we find the processes extending in number and in refinement. The principle of the division of labour is introduced: the tasks before assigned to one and the same organ being now apportioned among different sets of organs, the quality of the work is in the same proportion improved. In the higher classes of animals, the separation of offices becomes still more complete, and the products of one set of organs are passed on to the next in regular succession."

[8] See Roget's contribution to the "Bridgewater Treatises": *Animal and Vegetable Physiology Considered with Reference to Natural Theology*, 2 vols. (London: Pickering, 1834), vol. 2, pp. 104–106, where he substantially repeated on division of labour and "the manufacture of nutriment" what he had written in 1826 and added nothing more.

[9] Milne-Edwards: "Organisation" (n. 3 supra), pp. 340–341. In the article "Nerfs" (n. 3 supra) the argument is of course mainly restricted to the functions of the nervous system, but Edwards emphasizes that "Ce que nous venons de voir pour le système nerveux a également lieu dans toutes les parties de l'économie animale." He also makes clear the connection with political economy: "La nature, toujours économe dans les moyens qu'elle emploie pour arriver à un but quelconque, a donc suivi dans le perfectionnement des êtres *le principe si bien développé par les économistes modernes*, et c'est dans ses oeuvres aussi bien que dans les productions de l'art, que l'on voit les avantages immenses qui résultent de la *division du travail*" (p. 534; the underlinings are mine).

[10] "Organisation" (n. 3 supra), p. 345.

[11] Milne-Edwards: *Eléments de zoologie, ou leçons sur l'anatomie, la physiologie, la*

classification et les moeurs des animaux (Paris: Crochard, 1834), p. 8. This is the course he taught at the *Ecole* in 1832–1833 (see p. vii), in which it seems appeared for the first time the complete phrase "*division du travail physiologique*". The course of 1831–1832 had been published by lithographic process from the notes taken by some of his students: Ecole centrale des Arts et Manufactures, *Cours d'anatomie, de physiologie et de zoologie*, par M. Henri Milne Edwards, rédigé par M.M. Camille Laurens, Mamet, L'Amulonnière et Auffroy [Milne-Edwards is given there the title of "*professeur d'histoire naturelle industrielle*"]. The *Eléments* were much influential, being used as a textbook in the *lycées* at least for half a century (see 12th edition, 1877; it may not have been the last edition).

[12] 4 vols. (Paris: Roret, 1834–1840).

[13] *Ibid.*, vol. 1, pp. 226–229.

[14] " . . . cette tendance au perfectionnement de l'organisme par la division du travail physiologique sur laquelle j'ai appelé l'attention": "Recherches zoologiques faites pendant un voyage sur les côtes de la Sicile. III. Observations sur la circulation", *Annales des Sciences naturelles*, 1845, 3rd, ser., **3**: 257–307; p. 287.

[15] H. Milne-Edwards: *Introduction à la zoologie générale ou Considérations sur les tendances de la Nature dans la constitution du règne animal (Première partie)* (Paris: Masson, 1851). No Second part was ever published.

[16] "Autant la nature est prodigue de la variété dans ses créations, autant elle paraît économe dans les moyens qu'elle emploie pour diversifier ses oeuvres"; *ibid.*, p. 9.

[17] *Ibid.*, p. 21.

[18] "L'influence du volume d'un organe ou instrument physiologique sur la quantité des produits qu'il peut fournir, ou pour employer ici *le langage de la technologie*, l'influence de la masse des matières sur le *rendement* de la machine que ces matières constituent, est facile à constater" (*ibid.*, p. 23. The underlining is mine). At the time Milne-Edwards was writing, "technology" did not denote a piece of machinery or a technical process, but the project of a new science for the rational use of technical apparatusses and processes to achieve the best possible economic return. See Jacques Guillerme & Jan Sebestik: "Les commencements de la technologie", *Thalès*, 1966, **12**, pp. 1–110.

[19] Milne-Edwards (n. 15 supra), p. 26.

[20] *Ibid.*, p. 29.

[21] *Ibid.*, pp. 35–36.

[22] *Ibid.*, pp. 157–158.

[23] *Ibid.*, p. 158.

[24] Though we know that he rejected Darwin's explanation of evolution through natural selection. See H. Milne-Edwards: *Rapports sur les progrès des sciences zoologiques* (Paris: Imprimerie impériale, 1867), p. 428, n. 1. However, he was among the supporters for Darwin's election to the Académie des sciences.

[25] The family came from Jamaica, established itself in England and later at Bruges, in Belgium where Henri-Milne Edwards was born in 1800, and moved to Paris after the invasion of 1814.

[26] Adam Smith (R.H. Campbell, A. Skinner & W.B. Todd (eds.): *An Inquiry into the Nature and Causes of the Wealth of Nations*, 2 vols. (Oxford: Clarendon Press, 1976) vol. I, pp. 4–8. Adam Smith was also aware of other, detrimental, consequences of division of labour; however it is beyond our object to discuss this here.

[27] Milne-Edwards (n. 15 supra), p. 35.
[28] *Ibid.*, p. 22.
[29] *Ibid.*, p. 36.
[30] *Ibid.*, p. 60. Also, p. 68: "La tendance générale de la nature est de varier de plus en plus les instruments physiologiques dont la réunion constitue l'organisme animal à mesure qu'elle produit des espèces plus parfaites."
[31] These quotations are taken from L.G. Stevenson: "Anatomical Reasoning in Physiological Thought," pp. 27–38 of C.McC. Brooks & P.F. Cranefield, (eds.): *The Historical Development of Physiological Thought* (New York: Hafner, 1959); see p. 35.
[32] "Dans la seconde partie de ce travail, nous présenterons la description anatomique des divers organes de la circulation; mais il était nécessaire de résoudre d'abord la question physiologique par des expériences sur les animaux vivants; l'anatomie seule ne pouvait nous permettre de fournir des lumières suffisantes pour comprendre et expliquer cette importante fonction" (Offprint, p. 32). The paper was published in the *Annales des Sciences naturelles* in the summer of 1827.
[33] "Quant aux expériences physiologiques . . . elles sont sûrement importantes, et peut-être ont-elles aidé les auteurs de la découverte des faits qu'ils ont si bien fait connaître; mais le résultat n'en pouvait être déduit et bien conçu qu'après les recherches anatomiques" (Offprint, p. viii). However, in a necrological memoir on Audouin, Milne-Edwards makes clear that it is thanks to the personal support of Cuvier that this research piece earned them that year the Academy's prize for experimental physiology. See H. Milne-Edwards: *Notice sur la vie et les travaux de Victor Audouin* (Paris: 1850), p. 11.
[34] Such as the transformation of "dominating characters" into "predominant characters". See Milne-Edwards (n. 15 supra), Chapter X.
[35] This aspect is developed as a chapter: "L'économie politique d'une théorie hégémonique en histoire naturelle: le cuviérisme", in C. Limoges, *Etudes d'histoire de la biologie* (Montréal: Presses de l'Université du Québec, forthcoming).
[36] Milne-Edwards (n. 15 supra), p. 36.
[37] *Wealth of Nations*, Chapter 3, "That the Division Labour is Limited by the Extent of the Market."
[38] C. Limoges: "Introduction", pp. 7–22 of C. Linné: *L'équilibre de la nature*, trans. B. Jasmin (Paris: Librairie Philosophique J. Vrin, 1972).
[39] Milne-Edwards (n. 15 supra), p. 13.
[40] Bernard Balan has pointed out in his very lucid analysis of Milne-Edwards' ideas that the very notion of a division of physiological labor, in contrast with Cuvier's emphasis on the significance of the "conditions of existence", substituted a viewpoint centered on the "internal technology" of the organism. See *L'ordre et le temps. L'anatomie comparée et l'histoire des vivants au XIXe siècle* (Paris: Librairie Philosophique J. Vrin, 1979), p. 298. This would have removed the attention from the consideration of the market-like living environment. However the extent to which Cuvier's concept of "conditions of existence" really referred to environmental conditions is debatable. See C. Limoges: "L'économie naturelle et le principe de corrélation chez Cuvier et Darwin", *Revue d'histoire des sciences*, 1970, **23**, pp. 35–48.
[41] Milne-Edwards (n. 15 supra), p. 12.
[42] *Ibid.*, p. 21.
[43] *Ibid.*, p. 52.

[44] *Ibid.*, p. 23.
[45] *Ibid.*, p. 36.
[46] *Ibid.*, p. 38.
[47] See his *Leçons sur la physiologie et l'anatomie comparée des hommes et des animaux*, 14 vols. (Paris: Masson, 1857–1881): "On voit alors la division du travail s'introduire de plus en plus complètement dans l'organisme: chaque acte vital tend à s'effectuer au moyen d'un instrument particulier, et c'est par le concours d'agents dissemblables que le résultat général s'obtient. Or les facultés de l'animal deviennent d'autant plus *exquises* que cette division du travail est portée plus loin; quand un même organe exerce à la fois plusieurs fonctions, les effets produits sont tous *imparfaits*, et tout instrument physiologique remplit d'autant mieux son rôle que ce rôle est plus spécial" (vol. 1, p. 19). " . . . plus cette division est portée loin, plus les produits ont de valeur, plus la machine vivante est *parfaite*" (vol. 14, pp. 279–280; the underlinings are mine.)
[48] Milne-Edwards (n. 15 supra), p. 177.
[49] *Ibid.*, p. 118: "Mais à côté de la concession ainsi faite de besoin de *variété* qui semble exercer une influence si puissante sur la création toute entière, nous voyons encore les effets de cette *tendance à l'économie* dont l'étude du perfectionnement physiologique nous avait déjà fourni tant de preuves." It is no surprise that Darwin marked the passage in his copy and wrote in both margins: "poor!".
[50] *Ibid.*, Préface, p. ii.
[51] *Notebook E*: 100. The paper was Milne-Edwards: "Mémoire sur la distribution géographique des Crustacés," published in 1838 in *L'Institut*, where Darwin read it (pp. 290 sq.), and in the *Annales des Sciences naturelles*, 1838, **11**: 129–174. *Notebook* E, p. 25 refers to another article by Milne-Edwards published in the same journal in 1838; *Notebook* B, p. 112, in 1837, Darwin refers to Milne-Edwards: "Sur l'organisation de la bouche chez les Crustacés suceurs", *Annales des Sciences naturelles*, 1833, **28**: 78–86. *Notebook* D, p. 52, (1838) quotes William Sharp MacLeay referring to Milne-Edwards's *Histoire naturelle des Crustacés.*
[52] In *Annales des Sciences naturelles*, 1844, 3rd ser. Zoologie, **1**: 65–99. The date of reading is taken from Darwin's notebook on the books he read, Cambridge University Library, DAR 119, fol. 17.
[53] Reading completed through volume 3 on the 30th of January 1847 (volume IV includes plates only). Darwin's copy, kept in the Darwin Collection at the Cambridge University Library bears his marks, particularly on pp. 226–227 in volume I, from which we have quoted above, where Milne-Edwards makes explicit his views, distinct from those of Cuvier, on the natural grouping of organisms. Darwin manifested his agreement on a penciled paper note pinned in that volume: "226–8 on 2 methods of classification: that of Cuvier *impracticable* (*very good sentence*)." Darwin had also had taken note of these views when he read and annotated the detailed summary given by George Johnston: "Miscellanea Zoologica", *Magazine of Zoology and Botany*, 1837, **1**: 368–382. See the copy kept in the Darwin Collection, Cambridge University Library; esp. pp. 374–375. The reading of the *Magazine* is mentioned by Darwin in *Notebook* C, p. 275.
[54] Underlining here is mine.
[55] Cambridge University Library, DAR 72, fol. 117–121.
[56] C. Darwin: Letter to J.D. Hooker, June 8, 1858, *in* Francis Darwin & A.C. Seward (eds.): *More Letters of Charles Darwin: A Record of His Work in a Series of Hitherto*

Unpublished Letters, 2 vols. (London: John Murray, 1903), vol. 1, p. 109. See C. Limoges: "Darwinisme et adaptation", *Revue des questions scientifiques*, 1970, **140**: 353–374; p. 353.

[57] ". . . our principle of Divergence, which regulates the Natural Selection of variation . . . " *in* R.C. Stauffer (ed.): *Charles Darwin's Natural Selection, being the Second Part of His Big Species Book Written from 1856 to 1858* (London/New York: Cambridge University Press, 1975), p. 249.

[58] Ernst Mayr: "Darwin's Five Theories of Evolution", pp. 755–772 of David Kohn (ed.): *The Darwin Heritage* (Princeton N.J.: Princeton University Press, 1985), pp. 759–760.

[59] C. Limoges: *La sélection naturelle. Etude sur la première constitution d'un concept (1838–1859)* (Paris: Presses Universitaires de France, 1970), p. 131 sq. Darwin's complete argument is best summarized by David Kohn: "1. First there is an economical premise. A locality can support more life if occupied by diverse forms partitioning resources. This is the ecological division of labor. Thus specialization is an adaptive advantage to an organism. Hence natural selection, which explains the origin of all adaptation, favors the evolution of new specialized varieties. 2. The making of a new variety occurs sympatrically, that is, with parental and offspring forms inhabiting the same locale. Thus the making of varieties, which Darwin saw as incipient species, occurs by vigorous selection for specialization overcoming the swamping effects of crossing. 3. From the first fork of the branching phylogeny it is a matter or reiteration to generate all of classification. Simply put, niche within niche engenders group within group." D. Kohn: "Darwin's Principle of Divergence as Internal Dialogue," pp. 245–257 of D. Kohn (n. 58 supra), esp. p. 245.

[60] Darwin (n. 57 supra), p. 233.

[61] C. Darwin: *On the Origin of Species. A Facsimile of the First Edition*, Intr. Ernst Mayr (Cambridge, Mass.: Harvard University Press, 1964), pp. 115–116.

[62] C. Limoges: "Darwin, Milne-Edwards et le principe de divergence", *Actes du XIIe Congrès International d'Histoire des Sciences, Paris, 1968* (Paris: Librairie scientifique et technique Albert Blanchard, 1971), **8**: 111–115; p. 114. Also: Limoges (n. 59 supra), pp. 134–136. Since my first article on Darwin's principle of divergence, there has been a substantial literature devoted to the analysis of the genesis of this concept. See for instance: F.J. Sulloway: "Geographic Isolation in Darwin's Thinking: The Vicissitudes of a Crucial Idea", *Studies in History of Biology*, 1979, **3**: 23–65; E.J. Browne: "Darwin's Botanical Arithmetic and the "Principle of Divergence, 1854–1858", *Journal of the History of Biology*, 1980, **13**: 53–89; S.S. Schweber: "Darwin and the Political Economists: Divergence of Characters", *ibid.*, 1980, **13**: 195–289; D. Ospovat: *The Development of Darwin's Theory Natural History, Natural Theology and Natural Selection, 1838–1859* (Cambridge/London/New York: Cambridge University Press, 1981), esp. pp. 146–209; D. Kohn: "On the Origin of the Principle of Divergence", *Science*, 1981, **213**: 1105–1108; J. Browne: *The Secular Ark: Studies in the History of Biogeography* (New Haven: Yale University Press, 1983), esp. pp. 206–218; S.S. Schweber: "Facteurs idéologiques et intellectuels dans la genèse de la théorie de la sélection naturelle", pp. 123–142 of Y. Conry (ed.): *De Darwin au Darwinisme, Science et idéologie* (Paris: Librairie philosophique J. Vrin, 1983); S.S. Schweber: "The Wider British Context in Darwin's Theorizing", pp. 35–69 of D. Kohn (in 58 supra); D. Kohn: "Darwin's Principle of Divergence as Internal Dialogue", *ibid.*, pp. 245–257.

This is not the place to review this literature. It should however be emphasized that though some of these scholars have adopted different viewpoints on the precise timing and nature of the input provided by Darwin's readings of Milne-Edwards, the connection itself as we will see, remains unquestionable.

[63] ". . . at the time [the writing of the 1844 *Essay*] I overlooked one problem of great importance; and it is astonishing to me, except on the principle of Columbus and his egg, how I could have overlooked it and its solution. The problem is the tendency of organic beings descended from the same stock to diverge in character as they become modified. That they have diverged greatly is obvious from the manner in which species of all kinds can be classed under genera, genera under families, families under suborders, and so forth; and I can remember the very spot in the road, whilst in my carriage, when to my joy the solution occured to me; and this was long after I came to Down." N. Barlow (ed.): *The Autobiography of Charles Darwin 1809–1882* (London: Collins, 1958), pp. 120–121.

[64] "It is to me really laughable when I think of the years which elapsed before I saw what I believe to be the explanation of some parts of the case; I believe it was fifteen years after I began before I saw the meaning and cause of the divergence of the descendants of any one pair." Letter to George Bentham, 19 June 1863, in Francis Darwin (ed.): *The Life and Letters of Charles Darwin*, 2 vols. (New York: Appleton, 1899), vol. 2, p. 211.

[65] See Cambridge University Library, DAR 128: in this notebook on the books he read, the last entry for 1852 is: "M. Edwards. Introduction Zoolog. générale 1851."

[66] D. Ospovat: (n. 62 supra).

[67] David Kohn: *ibid.*, p. 250.

[68] Cambridge University Library, DAR 205.9: 50; quoted in Ospovat (n. 62 supra), p. 267, n. 49.

[69] Darwin (n. 61 supra), pp. 115–116.

[70] Darwin's manuscript comment in the margin of p. 126 of his copy of the *Introduction à la zoologie générale*.

[71] Limoges (n. 62 supra), p. 114; and (n. 59 supra), p. 136.

[72] The best and most complete study on Durkheim is Steven Lukes: *Emile Durkheim. His Life and Work: A Historical and Critical Study* (London: Penguin Books, 1975).

[73] E. Durkheim: *De la Division du Travail Social* (Paris: Presses Universitaires de France, 1960), p. 1: "Sans doute, dès l'Antiquité, plusieurs penseurs en aperçurent l'importance; mais Adam Smith est le premier qui ait essayé d'en faire la théorie. C'est d'ailleurs lui qui créa ce mot, que la science sociale prêta plus tard à la biologie." Moreover, he knew of course of the views of Comte, Spencer, Espinas and Tönnies and discussed them.

[74] *Ibid.*, preface to the first edition, pp. xliii–xliv.

[75] *Ibid.*, pp. 3, 260.

[76] *Ibid.*, p. 3. Durkheim mentions here the works of Wolff, von Baer and Milne-Edwards.

[77] *Ibid.*, p. 4.

[78] *Ibid.*, p. 150.

[79] *Ibid.*, p. 149.

[80] *Ibid.*, p. 100.

[81] *Ibid.*, p. 154.

[82] *Ibid.*, p. 157.

[83] *Ibid.*, p. 212.

[84] *Ibid.*, pp. 236–244.
[85] *Ibid.*, pp. 248–250.
[86] *Ibid.*, p. 253.
[87] *Ibid.*, p. 259.
[88] "Tout se passe mécaniquement. Une rupture d'équilibre dans la masse sociale suscite des conflits qui ne peuvent être résolus que par une division du travail plus développée: tel est le moteur du progrès." *Ibid.*, p. 253. Despite the physicalist language used here, it remains clear that the explanation had a biological foundation: there is no principle of divergence for inert objects.
[89] Lukes (n. 72 supra), pp. 34–35.
[90] *Ibid.*, pp. 167–168. Lukes points out (p. 168, n. 51) that Durkheim in *La division du travail social* "takes material density to be an index of moral density, but renounces this procedure" in the *Règles de la méthode sociologique* (n. 92 infra).
[91] Published in 1895. See E. Durkheim: *Les règles de la méthode sociologique* (Paris: Presses universitaires de France, 1973), p. 113, n. 1.
[92] This is not to imply that Durkheim borrowed the entire theory of evolution by natural selection. Paul Hirst has pointed out that Durkheim paid no attention to Darwin's concept of random variation, for instance. But this is not enough to assert that Durkheim "in fact did not adopt a simple Darwinist explanation". Hirst ignores the fact that what Durkheim borrowed is the principle of divergence, which already embodied the concept of a division of ecological labour and implied random variations. Moreover Durkheim's solution in no way denies the assumption of these random variations. More generally, the weakness of Hirst's viewpoint comes mainly from the fact that he interprets the *Division du travail social* from the standpoint of a work published later, the *Règles de la méthode sociologique*. See P.Q. Hirst: "Morphology and pathology. Biological analogies and metaphors in Durkheim's *The Rules of Sociological Method*", *Economy and Society*, 1973, **3**: pp. 1–34.
[93] R.A. Nisbet (ed.): *Emile Durkheim* (Englewood Cliffs, N.J.: Prentice-Hall, 1965), p. 37.
[94] Durkheim (n. 73 supra), p. 4.
[95] Lukes (n. 72 supra), p. 171.
[96] Hirst (n. 92 supra), p. 19.
[97] See for instance David O. Edge: "Technological metaphor", pp. 31–64 of D.O. Edge & J.N. Wolfe (eds.): *Meaning and Control. Essays in Social Aspects of Science and Technology* (London: Tavistock Publications, 1973).
[98] M. Ruse: "The Nature of Scientific Models: Formal v. Material Analogy", *Philosophy of the Social Sciences*, 1973, **3**: 63–80.
[99] C. Ménard: "La machine et le coeur. Essai sur les analogies dans le raisonnement économique", pp. 137–161 of A. Lichnerowicz, F. Perroux & G. Gadoffre, (eds.): *Analogies et connaissance*. Tome II. – *De la poésie à la science* (Paris: Maloine, 1981).
[100] I.B. Cohen: *The Newtonian Revolution, with illustrations of the transformation of scientific ideas* (Cambridge/London/New York: Cambridge University Press, 1980), esp. Chapter 4.

THEODORE M. PORTER

11. FROM QUETELET TO MAXWELL: SOCIAL STATISTICS AND THE ORIGINS OF STATISTICAL PHYSICS

The peregrinations of statistics constitute one of the weightiest and most unpredictable chapters in the history of the transmission of ideas between the natural and social sciences. Mathematical statistics has long been idealized as a possible means for capturing the holy grail of the sciences of man, quantification, and probability theory was central to the earliest sustained effort in this direction, Condorcet's social mathematics. Laplace's injuction in the *Philosophical Essay on Probabilities* to "apply to the political and moral sciences the method founded upon observation and upon calculus, the method which has served us so well in the natural sciences,"[1] was already a commonplace in 1814, and was frequently invoked throughout the nineteenth century. Yet the migration of mathematics from the hard to the soft sciences is only a part of the career of statistical thinking since the time of Laplace. Equally important, and perhaps more impressive, is the role of that prominent nineteenth-century social science, "statistics", in facilitating the application of probabilistic mathematics to the biological and physical domains. This aspect of the transmission of ideas is illustrated by James Clerk Maxwell's observation that "atomists" of his own day had "adopted a method which is, I believe, new in the department of mathematical physics, though it has long been in use in the section of statistics." The strategy of census-takers, according to Maxwell, had opened a new path in the physical theory of gases.[2]

Laplace's dictum has been enshrined by generations of natural as well as social scientists, including many would-be Newtons who believed themselves to have found in some aspect of physics, chemistry, or biology the key to the theoretical problems involved in studying the affairs of humanity. Interdisciplinary influences of the sort implied by Maxwell seem more surprising, perhaps because some variant of Auguste Comte's hierarchy of the sciences has come to be taken for granted. Indeed, if we were speaking of the reduction of theories (which Comte was not), a rigid hierarchy of disciplines would make good sense. It is hard to imagine a theory of physics formally grounded in principles of social

I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 345–362.

behavior. The sciences of human societies, however, have been influenced far more often, and deeply, by analogies and models from the natural sciences than by attempts at reduction.[3] This holds especially for those cases in which probability and statistics are involved. To be sure, there is reason to expect interdisciplinary flow even of analogies to proceed down rather than up the Comtean hierarchy, since the physical domain has proven a readier source of tractable problems than the biological or social. This, however, is no more than a generalization, true contingently but not essentially. There is no reason that the social disciplines should not occasionally provide models for the physical and biological sciences. In truth they have, as I hope to show in the present essay.[4]

Social physics was invented by Comte, who intended by this title nothing more than the mature form of social science. Like mathematics, astronomy, physics, chemistry, and physiology, the study of society was obliged to pass through theological and metaphysical stages before it could become a true positive science, or *physique sociale*. Moreover, social physics was to be the last science to reach the positive stage, for its object was the most complex and difficult domain presented by nature, and it was dependent on the understanding of simpler phenomena such as the seasons and human diseases, which fell within the province of the other sciences. Nevertheless, Comte did not idealize the method of physics or physiology as universally applicable, and he scorned the application to society of tools such as mathematics which had been integral to the progress of science in other domains. The proper method of social physics was historical, the study of the development of social organization as manifested in human institutions.[5]

Whereas Comte's *physique sociale* harks back to an older and more general sense of the term physics (as in D'Alembert's "Discours préliminaire" to the *Encyclopédie*), the Belgian astronomer and statistician Adolphe Quetelet adopted the expression with the specific intent of indicating the dependence of social science on the methods and concepts of the physical sciences. There is reason to suppose that Comte gave up the title social physics for sociology because of Quetelet's theft of it, and it is clear that he had nothing but disdain for this attempt to base social science on that frivolous agent of social quantification, statistics.[6] Quetelet, then, is the undisputed founder of the imitative form of social physics[7] – which, incidentally, was born in 1831 as *mécanique sociale*, the social correlate of *mécanique céleste*, then renamed *physique sociale* in 1835. Quetelet's social physics could be mapped onto celestial mechanics in a one-to-one correspondence without changing anything but the names, and often not even these. He wrote in 1834:

The great problems of population dynamics, like those of the motion of celestial bodies, can be solved – and, what is most remarkable, there is a surprising analogy between the formulas employed in these calculations. I believe that I have achieved to some extent what I have long said about the possibility of founding a social mechanics on the model established by celestial mechanics – to formulate the motions of the social body in accordance with those of celestial bodies, and to find there again the same properties and laws of conservation.[8]

According to Quetelet, society may be characterized as a unitary body moving through the medium of history along a path determined by two kinds of forces, constant and perturbational. The constant forces act continuously, and tend to produce perfectly stable motion which, however, the perturbational forces are forever deflecting. The social physicist must solve this problem with the tools of astronomy. First he determines the path produced by the constant forces, whose social identity is ambiguous but which correspond to the gravitational attraction of the sun. Then he adds to this the force produced by other objects, such as planets that pass by from time to time. The complex effects of third and fourth bodies had kept Quetelet's mentor, Laplace, occupied for years calculating the paths of the planets. Quetelet proposed that the social physicist should restrain his ambition for awhile and, imitating Kepler, seek first the path the social body would follow in the absence of perturbations.

Quetelet's characterization of constant forces as natural forces, and of perturbational as those arising from the influence of man, fit poorly with this astronomical metaphor, since he also maintained that the forces of man were the main source of human progress. This, however, was not necessarily cause for despair. Imperfect or incomplete analogies can be sources of creative tension in science. Some of the most important developments in nineteenth-century statistical thinking arose from the need to work out the novel implications of a mathematical analogy that arose in a new context of application. But Quetelet adhered to the school of Procrustes, and the consequences of imperfections in his analogies he left to others.[9] He would not allow himself to become ensnared in details. Rather, he proceeded bravely, enunciating a firm plan for moving this scheme out of the realm of pure speculation into that of exact science. The key to social physics was to be *l'homme moyen*, the average man. This was a purely statistical construct, whose traits were the mean of every attribute represented in the population. The average man, Quetelet proposed, was the social correlate of the center of gravity of a planet. Social physics could ignore the diverse attributes of human individuals and seek simply to characterize the change (*mouvement*) of this idealized being over time.

The physical dimensions of the average man could be calculated directly from actual measurements of individuals. Moral attributes, however, seemed far more interesting to Quetelet and his contemporaries, who were deeply troubled by the apparent increase of crime, suicide, prostitution, and of the standard index of public immorality, the ratio of births out of wedlock to total births. Ascertaining these moral dimensions of the average man was evidently more difficult, but Quetelet believed that he had a solution. In principle, he observed, the best strategy would be to make use of experiments. Thus, to attain a measure of the courage possessed by a given individual, the social physicist need only place him in a great number of situations in which an act of courage was called for, and count the instances in which the appropriate response was elicited. Since the equality of cause and effect is the first principle of all scientific philosophy, physical actions necessarily provide accurate measures of the moral and intellectual dispositions that produce them.

This laboratory technique, of course, was only a solution in principle, and not a feasible program for social science. Nevertheless, Quetelet hastened to point out, most of the information it would yield was not actually required by the social physicist, since societies rather than individuals were his principle concern. Hence the private laboratory was unnecessary. Instead, the investigator needed only to record the results of experiments carried out in the great laboratory of nature. If, for instance, he desired to know the quantity of courage present in a given society, if would suffice to arrange that acts of courage be systematically recorded in a uniform manner. Quetelet readily acknowledged that this information was not in fact being collected, but he saw no reason why it couldn't be. Moreover, a great range of materials about the moral condition of the people was being collected, some of it of even greater importance than courage. Murders, thefts, suicides, births, deaths from various causes, marriages, arrests for prostitution, and other events began, between 1820 and 1840, to be systematically counted and recorded. On the basis of these tallies, Quetelet was able to assign numbers to moral attributes. He endowed his average man not only with height and weight, but also with propensities for crime, suicide, and marriage, equal to the number of those acts committed divided by the population.[10]

Social physics was formally a science of movement, of the path of the social body through the multiple physical and moral dimensions of history. With this in mind, Quetelet issued various pronouncements about the historical implications of his scientific researches. For example, he enunciated as a law of history the proposition that the course of the social body is ineluctably progressive, entailing the gradual triumph of *l'homme intellectuel* over *l'homme physique*. He also worried publicly about how this desideratum would be realized – whether by steady, continuous exertion, the surest and most desirable course, or by violence and revolution. These might perhaps be necessary to overcome the obstacles of political inflexibility, but, like collisions in physics, they always involve the loss of living, moral force, the life blood of progress. Time and again he asked rhetorically whether perturbational forces could compromise the stability of the social system, at the same time pointing hopefully to Laplace's demonstration that they did not undermine the stability of planetary movements.

The concrete results that Quetelet achieved through his statistical researches were never brought to bear on historical issues of this sort, by himself or anyone else. As a practical statistician, collecting and comparing results, he was among the most competent workers of his generation, but social physics was never more than a host of glib analogies and a few sine curves fitted to the variation of births or suicides by month. Indeed, Quetelet's entire project of mathematization in the social sciences was, in his own day, no more than a dream. This was true even of his attempt to incorporate the analytical results of mathematical probability into the numerical method of statistics. Although he inveighed repeatedly against the absurd blunders of his fellow statisticians, which he attributed to their ignorance of probabilistic error analysis, his own writings on social statistics may be searched in vain for even a single computation of a probable error.

Quetelet's most celebrated results in social physics, his "statistical laws", involved neither higher mathematics nor the dimension of history. A statistical law, in its most elementary form, consisted simply of the proposition that some particular class of events exhibits constant numbers or proportions from year to year, and will continue to do so in the near future. The great ancestor of nineteenth-century statistical laws was John Arbuthnot's discovery around 1710 that male births exceeded female births by approximately one twentieth every year, seemingly without exception. This bit of information came to be seen during the eighteenth century as one of the most compelling illustrations of divine wisdom and beneficence – though the regularity of deaths by age was also much admired. Nineteenth-century readers received more diverse amusement. First came Laplace's revelation, stunning in its triviality, that the same number of undeliverable letters reached the dead letter office in the Paris postal system each year.[11] Even more impressive were those "moral laws" that Quetelet made famous, the laws of crime and moral turpitude. Not only was the number of suicides and murders the same every year, but even the number of those committed with guns or knives, by poison or drowning.

These numbers were used by nineteenth-century authors, including Quetelet, to learn about the tendencies of various lands and groups. Women, it seemed, liked to drown, asphyxiate, or poison themselves, men to shoot or knife themselves. Parisians were fond of drowning, Czechs and Saxons of hanging, though young Parisians were somewhat inclined to hang themselves, old Parisians to shoot themselves. Suicide was more common in the north of France than the south, and while residents of Berlin, Hamburg, and Paris preferred to commit suicide in the summer, the winter season was equally fashionable in London.[12] Quetelet sometimes put forward theories to explain these diverse facts, but as social physicist, he was satisfied to revel in their consistency from year to year. What particularly impressed him, and many of his contemporaries, was that such lawlike regularity should be displayed by the most disorderly, anti-social, irrational, and malicious products of deranged human passions. This presented a splendid paradox, and involved a moral victory of the statistician over the forces of disorder. To violate a law was to obey a law; to murder a statistician would be to prostrate oneself before the exact laws of statistical science.

In the same vein, social physics promised to provide a foundation for the reform of society under the aegis not of the unruly mob, but the mathematically-trained statistician. As Quetelet argued, in a frequently quoted passage, the "constancy with which the same crimes are annually reproduced in the same order and receive the same penalties in the same proportions," was like a budget, paid with a "frightening regularity," or even a tribute "that man acquits with greater regularity than that which he owes to nature or to the state treasury." This circumstance might seem discouraging, he went on, since the individual appears to be somehow impelled to crime, even against his will. Instead, we ought to be consoled by it, for since crime is to be regarded as the result of social conditions, and not of the depravity of human individuals, it ought to be possible to improve men "by modifying their institutions, their customs, the state of their enlightenment, and, in general, all that influences the manner of their existence."[13] Such improvement would be guided by the social physicist, either by means of his computations of the trajectory of the social body or, less abstrusely, through statistical analysis of the effects of various legislative measures on the level of criminal activity.

For all its cosmic optimism, Quetelet's social science was rooted as much in a post-revolutionary nervousness about the possibility of violent revolutionary change as in consciousness of the possibilities of progress. The great statistical movement of the 1830s and 1840s in Great Britain was similarly motivated, and its early champions exhibited a special concern with the physical and moral condition of the poor. But the early "statists", as practioners of the social science of statistics in Britain were called, entertained few ambitions for the development of theoretical knowledge, and preferred to ground their science in the certainty of its facts.[14] Quetelet helped awaken greater scientific ambitions, and promoted an increased emphasis on theory. Whereas the British statistical movement had been founded by empiricist critics of deductive political economy,[15] the doctrine of statistical law was embraced by champions of scientific abstraction in social doctrine. Here, again, statistics was inseparable from political argument, and inevitably politics played a role in the understanding that was put forth of statistical method.

That understanding was liberal, in the British sense. In contrast to Quetelet, a bureaucratic liberal, who hoped that numbers would enable the state to guide social progress, his most enthusiastic British followers held that statistics showed how little the state could accomplish. The idea of statistical law was propagated in Great Britain most effectively during the 1850s and 1860s, when the country appeared at last to be freed from the menace of revolution and when, in the wake of the Corn Law debates, it seemed clear to liberals that the remarkable progress of manufactures and commerce since the time of Smith had occurred despite, not because of, government activity. These were the years of liberalism triumphant. Adam Smith had shown that the economy functioned best without central direction, and now the laws of the meddling state seemed at last to be falling before the natural laws of economy and society.

Some sense of social determinism was almost presupposed by that distinctively nineteenth-century idea of "social science". Even Quetelet had denied that government could alter the general path of the social body, though it could correct deviations. Social theorists as different as Karl Marx and Herbert Spencer expected as the outcome of history the withering away of the state. But this could happen in a variety of ways, and British liberal theorists felt almost no need for public or collective action to advance this process. Accordingly, they enlisted statistics in the campaign to demonstrate that society was self-regulating, and that history was moving ineluctably towards the increase of private freedom. The economist William Newmarch, for example, maintained that the role of government was becoming increasingly circumscribed, and that official support for statistics had arisen from a perceived need to understand "the composition of the social forces which, so far, Governments have been assumed to control but which now, most men agree, really control Governments." State power over social forces, he wrote, is pure illusion, much like the control magicians had previously claimed over the weather. In reality, he proclaimed, "all attempts at making or administering laws which do not rest upon an accurate view of the social circumstances of the case, are neither more nor less than imposture in one of its most gigantic and perilous forms." Crime, education, taxes, wages – indeed, "every topic from the greatest to the least" – can no longer be dealt with according to legislative caprice, for all "have been found to have laws of their own, complete and irrefragable."[16]

The best known proponent in Great Britain of Quetelet's ideas was Henry Thomas Buckle, the first volume of whose *History of Civilization in England* was the hit of the 1857 literary season. The book went through scores of printings and abridgements in the major European languages, and was greeted often with a level of acclaim that modern readers find difficult to understand. Charles Darwin, for example, called Buckle the best writer in the English language, and his book the most important of the century. James Clerk Maxwell read the book within a few months of its publication and was sufficiently impressed to comment on it in a letter to his friend and subsequent biographer Lewis Campbell: "a bumptious book, strong positivism, emancipation from exploded notions, and that style of thing, but a great deal of actually original matter, the true result of fertile study and not mere brainspinning."[17]

Buckle's aim was of a familiar sort, to raise history to the status of a proper science. The problem with historians, according to Buckle, was that they had so wedded themselves to the narration of trivial political or ecclesiastical happenings that they were unable to see the broader picture – to recognize the laws of whole societies that govern unfailingly the seemingly chaotic behavior of individual actors. So little is expected of them, wrote Buckle, that "any author who from indolence of thought or natural incapacity, is unfit to deal with the highest branches of knowledge, has only to pass some years in reading a certain number of books, and then he is qualified to be an historian."[18]

Buckle's proposed science of history might be characterized as a variant of social physics, although he was never active as a natural scientist and made no use of mathematics. He had originally set out to write a general history of the world, putting forth a thesis beloved by Comte and Quetelet, that the fundamental cause of human progress was the advancement and diffusion of knowledge. After a decade of study he became convinced that the world was a bit large, and that he would have to confine his ambitions. He resolved instead to write a history of England, with the same main thesis and this secondary one: that the natural laws of the development of society are most clearly revealed in the history of England, since it of all countries had been least subject to destructive perturbations. There is, in other words, a natural course of social evolution, somehow inherent in the nature of things, which forms the object of the greatest interest to history. Like every science, after all, history must be concerned with regularities, and indeed with laws. Deviations from the normal path are of secondary interest, and can be attributed to the influence of perturbing agencies.

In this way, the history of every country became a perpetual striving to be like Victorian England. Unfortunately, all fell short – France because of too much reliance on the state, Scotland and Spain on account of the power of the clergy, Germany because of the severe concentration of knowledge in its universities, and the United States because of its excessive diffusion. Buckle never succeeded in putting the history of England to paper, because he died before completing the comparative histories of these mutant countries, his "general introduction" to the greater work. Still, he gave sufficient information to furnish a glimpse of the unperturbed course of social development. He wrote: "No great political improvement, no great reform, either legislative or executive, has ever been originated in any country by its rulers." Further, "every great reform which has been effected has consisted not in doing something new, but in undoing something old."[19] Buckle held that society was inherently progressive, but that public institutions such as state and church tended always to perpetuate outdated customs and relationships, and hence to obstruct its natural improvement. After centuries of struggle, society was at last emerging victorious in the most advanced country in the world, England, and throwing off the constraints of these conservative powers. Unshackled from these old institutions, the possibilities for progress were limitless.

Buckle justified his enterprise by citing examples of statistical regularity. The uniformity in annual returns of crime, suicide, and dead letters, he argued, proved that the course of social development was determined by general causes, and that all attempts to obstruct their operation were doomed to failure. Individuals, then, could achieve little, and the record of the deeds of kings and bishops was of only minor interest. The regularity of statistics indicated also the primacy of society over state. "From the circumstance that the discrepancies [from absolute uniformity] are so trifling," he wrote, "we may form some idea of the prodigious energy of those vast social laws which, though constantly interrupted, seem to triumph over every obstacle, and which, when examined by the aid of large numbers, scarcely undergo any sensible perturbation."[20]

The particular instances of regularity presented by Buckle had long been familiar to statistical initiates, since he lifted them directly from Quetelet. Buckle, however, stated this familiar doctrine with such aplomb and in so uncompromising a spirit that it became the subject of vigorous and wide-ranging public debate in Germany, Russia, the United States, and, especially, in Britain. Moreover, Buckle was not the only famous author to find inspiration in statistical returns. In 1850, Charles Dickens' *Household Words* discussed the statistical reports of the Registrar General in these terms: "Not content with making lightning run messages, chemistry polish boots, and steam deliver parcels and passengers, the savants are superseding the astrologers of old days, and the gipsies and wise women of modern ones, by finding out and revealing the hidden laws which rule that charming mystery of mysteries – that lode store of young maidens and gay bachelors – matrimony." Four years later, the more critical author of *Hard Times* had Tom Gradgrind disclaim responsibility for his theft on the ground that, as his father had always told him, such activities were the necessary consequences of unvarying statistical laws. By 1876, the same idea had been reduced to a tired cliché about progress and the regularity of statistics which George Eliot put in the mouth of a character in *Daniel Deronda*.[21]

Despite the widespread diffusion and frequent acceptance of this central doctrine of Quetelet's social physics, statistical law, its fruitfulness as a strategy for social science proved to be modest. Certainly, nobody – Quetelet included – had any idea how to calculate trajectories for the movement of the average man through the moral, physical and intellectual space defined by currently available statistics. Although the recognition of statistical regularities served prominent sociologists such as Durkheim as confirmation of the possibility of a science of the social, the proclamation of statistical laws led nowhere.

The fate of social physics was to become not the definitive science of society, but a model for certain aspects of natural science, among them a branch of the intended parent discipline, physics. The arguments of Buckle and Quetelet inspired a widespread willingness to assume the possibility, or even the necessity, of a statistical treatment of things. This assumption, in its most general form, holds that no matter how chaotic the behavior of individuals, statistical regularities can be expected to emerge in the mass. Science does not presuppose knowledge of every particular element, from which conclusions about more complex events are then deduced. It is possible to begin with the uniformities that characterize the mass, and from them to formulate general principles or to inquire about the particular events of which the large-scale regularities are composed.

This was the strategy adopted for the kinetic gas theory by James Clerk Maxwell. As he pointed out, there was no hope of deriving the macroscopic laws of gases by following the motions and collisions of millions of independent particles, for information about individual molecules was not available and the calculations would, in any event, be impossible. As an alternative, Maxwell proposed a different kind of social physics. In retrospect, at least, he often made this connection himself, as in his famous 1873 lecture to the British Association, where he argued that gas physicists had now adopted the methods of the social statisticians. Their problems were largely the same. The physicist cannot get at individual molecules, nor can he solve the formidably complex dynamical problem presented even by hundreds, much less by millions, of molecules. So also in social science: "The number of individuals is far too great to allow of their tracing the history of each separately, so that, in order to reduce their labour within human limits, they concentrate their attention on a small number of artificial groups. The varying number of individuals in each group, and not the varying state of each individual, is the primary datum from which they work. . . . " Since its problem was analogous, physics could make use of a similar solution. "The data of the statistical method as applied to molecular science are the sums of large numbers of molecular quantities. In studying the relations between quantities of this kind, we meet with a new kind of regularity, the regularity of averages, which we can depend upon for all practical purposes, but which can make no claim to that character of absolute precision which belongs to the laws of abstract dynamics."[22]

Statements like this one of Maxwell's became common during the late nineteenth century, and the nature of their message helps explain how it happened that the numerical science of society bequeathed its name to a modern area of applied mathematics. Francis Galton, founder of the English biometric school that developed modern statistics, published in 1869 an intricate comparison between the statistical regularities generated in a free society and those that could be explained by the behavior of his hypothetical agents of hereditary transmission, gemmules. Galton attributed the conspicuous similarities of parents and offspring to a combination of natural affinities and to the statistical principle that a large sample must always represent with considerable accuracy the composition of the greater population. Similarly, the economist Francis Edgeworth compared the behavior of prices to the results of moral and social statistics in his work on index numbers.[23]

Most impressive, perhaps, is Ludwig Boltzmann's use – independently, it seems – of precisely the same analogy Maxwell had employed, in the introduction to an important technical paper published by the Vienna Academy of Science. Boltzmann had evinced an extraordinary faith in the reliability of statistical regularity from the outset of his career, as is attested by his willingness to assume without comment or explanation that the probability of an arbitrary molecule being in a given state is precisely identical to the relative frequency of that state in a finite volume of gas at a given time. In his landmark paper of 1872, Boltzmann invoked the statistical laws of social science to convince his readers that the second law of thermodynamics, though based on averages, is by no means uncertain or merely probable. From other works it is clear that he became impressed with the lawfulness of social statistics through his reading of Buckle.[24]

Statistical reasoning was employed by these authors in part for reasons internal to their disciplines, and it is scarcely possible to establish that the statistical approach was only available to the kinetic theory or the study of heredity because it had already been developed in the context of social science. This story of interdisciplinary influences, however, does not end with the recognition in various disciplines of an abstract, statistical form of science. During the 1870s, Maxwell discussed the limitations of statistical knowledge in reference to what we might call a common context of physics and social theory. Debates in social thought helped him to formulate the implications of statistical reasoning for physics. He also derived from Quetelet and his followers a particular analytical tool, the "bell-shaped curve" or normal distribution,

$$(1/\sqrt{2\pi})e^{-x^2/2}.$$

This expression, which had become familiar because of its use for the reduction of observations in astronomy, geodesy, and related fields, was central to all statistical mathematics up to the time of Karl Pearson. In astronomy it had become known as the "error law", because astronomers understood it as regulating the errors made by careful observers. Its bell shape reflected the fact that small errors are common while large ones are comparatively rare, though even very large errors occur occasionally. Quetelet, ever eager to establish analogies between social physics and astronomy, had applied the error function to such objects as the distribution of chest sizes among Scottish soldiers. Although he imposed also the customary interpretation of this variation as error – suggesting that real, physical soldiers are flawed replicates of the average man – others were reluctant to join his dismissal of individuality as a product of imperfection. Instead, Quetelet's work led to a broader conception of the error law, which permitted its application even to genuine diversity in nature.

The incorporation of the error curve into the kinetic gas theory by Maxwell constituted just such an application, and we find that Quetelet's social physics was indeed the source for this new departure in physics. Maxwell evidently learned of Quetelet's use of the error distribution from a long review essay of one of his books by John Herschel, first published in 1850. Herschel fully accepted Quetelet's mathematical formula for the measures of Scottish soldiers, and with it, Quetelet's view that virtually all mass phenomena in nature or society could be expected to display the same distribuion. He proposed, following Quetelet, that the presence or absence of the error curve provided an accurate criterion for whether a given mean value was a "true mean" or a mere "arithmetic mean" (or average), and maintained that only the former was sufficiently reliable and informative for scientific work.

That Maxwell read and was impressed by this essay is clear from his letters. Its importance for him is indicated by the derivation of the error law (or Maxwell distribution) that he gave in his first paper on the kinetic theory. This was his epochal paper first read in 1859 – often regarded as the work that introduced probability theory into physics – where Maxwell showed that the distribution of molecular velocities cannot be ignored since it leads to different predictions from those previously published by Clausius. Maxwell's derivation was based on two assumptions: that the velocity coordinates along perpendicular axes are independent of one another, and that the three-dimensional distribution is spherically symmetric. The first assumption is expressed by writing the probability that a molecule has velocity x along one axis, y along the second, and z along the third, as the product of independent distribution functions,

$$\mathrm{f}(x)\ \mathrm{f}(y)\ \mathrm{f}(z)\ \mathrm{d}x\ \mathrm{d}y\ \mathrm{d}z.$$

Spherical symmetry implies that the total distribution is a function only of the absolute distance, that is

$$\mathrm{f}(x)\ \mathrm{f}(y)\ \mathrm{f}(z) = \phi(x^2 + y^2 + z^2).$$

The solution to this equation is the error curve, the principal term of the Maxwell-Boltzmann distribution in one dimension,

$$\mathrm{f}(x) = \mathrm{C}e^{-x^2/\mathrm{a}^2},$$

and likewise for y and z.[25]

The details of Maxwell's mathematics need not be discussed. The point here is that the very same derivation was given by John Herschel in his review of Quetelet's book.[26] That Herschel wrote this derivation in the context of an entirely different problem from Maxwell's – the error produced when balls are dropped at a target – indicates how rich were the analogies perceived among statistical objects, and how diversely the same formalism could be applied. Herschel had intended his derivation to be very general, for he, like Quetelet, believed that the error law was to be found throughout nature and society. Maxwell's replication of Herschel's mathematics would seem to have confirmed these expectations, and may be interpreted as an implicit declaration that the error law governs molecular velocities in a gas just as it governs an enormous range of other phenomena. The use of the error law had been intended by Quetelet to demonstrate that not only averages, but also deviations from the mean of human form and behavior, were subject to laws no less exact than those of celestial mechanics. Maxwell's use of Herschel's derivation, and hence of Quetelet's ideas, constitutes a revealing link between his work and the world view of the social statistical movement.

Although Maxwell began his work with an analogy, he was not limited to it, and in many respects his version of social physics was far more successful than Quetelet's. Using the error-curve velocity distribution, which was evidently his first result in the kinetic theory, Maxwell carried out a series of elegant combinatorial operations to reach a new formula for the mean free path of a gas molecule. The same formalism underlay most of Boltzmann's work, including his demonstration that the second law of thermodynamics was equivalent to the tendency of a system of molecules to move towards the most probable distribution of velocities.

There is yet more irony to the legacy of social physics than has appeared thus far. Quetelet's aim was to import the rigor of mechanics into the social sphere, and thereby to extend the domain of scientific certainty. Such, initially, was the consequence of his influence on gas physics, for it yielded a remarkable law for the distribution of velocities among gas molecules at a time when the truth of the kinetic theory, and indeed the very existence of molecules were still in doubt. A few years later, Maxwell became convinced that the applicability of statistical mathematics to this branch of mechanics had implications of a very different sort from anything anticipated by Quetelet or Buckle. Although Buckle's critics had already questioned his statistical determinism, physics provided a better context for working out this line of thought. However astonishing the regularities of statistics seemed to social theorists, the novelty of this approach for physics was not in its reliance on mathematics *per se* but in the special kind of mathematics it used. While there was no empirical reason to think the phenomena of heat less perfectly lawlike than those of mechanics or electricity or light, the introduction of probability into physics revealed to thinkers like Maxwell what observation alone could not: that certain macroscopic physical principles, among them the second law of thermodynamics, were no more than statements of probability, and not deterministic laws. Statistical summaries of innumerable events do not, Maxwell insisted, entail necessity. Even the molecular interactions themselves may, for all we know, be incompletely governed by physical laws.[27]

In the end, we might say, social physics served not to infuse social theory with the certainty of physical science, but to imbue natural knowledge with the uncertainty of the social. In this respect, however, the story of Quetelet, Buckle, and Maxwell only illustrates with special clarity a maxim which applies to most interdisciplinary influences. The beneficiary of these interactions is never a passive recipient, for the application of existing techniques to new subjects invariably requires some reinterpretation and frequently leads to changes in the techniques themselves. Analogies and metaphors, after all, are virtually never complete or perfect. In science, when new wine bursts old wineskins it is often to the advantage of both.

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NOTES

[1] Pierre Simon de Laplace: *Philosophical Essay on Probabilities* (1814), trans. F.W. Truscott and F.L. Emory (reprint, New York: Dover, 1951), pp. 107–108.

[2] James Clerk Maxwell: "Molecules" (1873), *Scientific Papers* (2 vols., Cambridge: Cambridge University Press, 1890), vol. 2, p. 373.

[3] See Theodore M. Porter: "Natural Science and Social Theory", in G.N. Cantor *et al.* (eds.), *Companion to the History of Modern Science* (London: Routledge; Chicago: The University of Chicago Press, 1990): 1024–1043.

[4] This argument is pursued more fully in my *The Rise of Statistical Thinking, 1820–1900* (Princeton: Princeton University Press, 1986). A similar point is developed in another context by Sharon Kingsland: *Modeling Nature: Episodes in the History of Population Ecology* (Chicago: The University of Chicago Press, 1985). The transmission of probabilistic ideas and methods from discipline to discipline is surveyed most comprehensively in Gerd Gigerenzer, Zeno Swijtink, Theodore Porter, Lorraine Daston, Lorenz Krüger, and John Beatty: *The Empire of Chance: How Probability Changed Science and Everyday Life* (Cambridge: Cambridge University Press, 1989).

[5] Auguste Comte: "Plan des travaux scientifiques nécessaires pour réorganiser la société" (1822), *Opuscules de philosophie sociale, 1819–1828* (Paris: Ernest Laroux, 1883), pp. 159–163, 172.

[6] Auguste Comte: *Cours de philosophie positive* (6 vols., Paris: Bachelier, 1830–1842), vol. 4 (1839), p. 7.
[7] Of course he had eminent predecessors in the effort to quantify social phenomena, though none, not even Laplace, imitated physics so devoutly as he did. See Keith Baker: *Condorcet: From Natural Philosophy to Social Mathematics* (Chicago: The University of Chicago Press, 1975), and Lorraine Daston: *Classical Probability in the Enlightenment* (Princeton: Princeton Unviersity Press, 1988).
[8] Adolphe Quetelet, letter to Sylvain von de Weyer, 1834, quoted in Académie Royale de Belgique: *Adolphe Quetelet, 1796–1874 (Mémorial)*, 4 vols. (Brussels: Palais des Académies, 1974), vol. 1, p. 91.
[9] His physicalist bias was later much criticized, especially in Germany. See articles by Theodore Porter, Ian Hacking, and Norton Wise in Lorenz Krüger, Lorraine Daston, & Michael Heidelberger (eds.): *The Probabilistic Revolution, vol. 1: Ideas in History*, 2 vols. (Cambridge, Mass.: A Bradford Book/The MIT Press, 1987), 351–425.
[10] More accurately, he divided the population by sex and assigned *l'homme moyen* a curve of penchants for those acts according to age. See Quetelet: "Sur la statistique morale et les principles qui doivent en former la base", *Nouveaux mémoires de l'Académie Royale des Sciences et Belles-Lettres de Belgique*, 1848, **21** (separate pagination).
[11] Laplace (n. 1 supra), pp. 61–62.
[12] See Quetelet: *Sur l'homme et le développement de ses facultés, ou essai de physique sociale*, 2 vols. (Paris: Bachelier, 1835); also Adolph Wagner: *Die Gesetzmässigkeit in den scheinbar willkührlichen menschlichen Handlungen vom Standpunkte der Statistik* (Hamburg: Boyes und Geister, 1864).
[13] Quetelet (n. 12 supra), vol. 1, pp. 8–11.
[14] See Michael Cullen: *The Statistical Movement in Early Victorian Britain: The Foundations of Empirical Social Research* (Hassocks, Sussex: Harvester Press Limited; New York: Barnes & Noble Books, 1975); Victor Hilts: "*Aliis exterendum*, or, the Origins of the Statistical Society of London", *Isis*, 1978, **69**: 21–43.
[15] See Lawrence Goldmann: "The Origins of British 'Social Science': Political Economy, Natural Science and Statistics, 1830–1835", *Historical Journal*, 1983, **26**: 587–616.
[16] William Newmarch: "Some Observations on the Present Position of Statistical Inquiry with Suggestions for Improving the Organization and Efficiency of the International Statistical Congress", *Journal of the Statistical Society of London*, 1860, **23** 362–369, esp. pp. 362–3.
[17] Letter to Lewis Campbell, 1857, in Lewis Campbell and William Garnett: *The Life of James Clerk Maxwell* (London: Macmillan, 1882), p. 294. For Darwin see Francis Darwin (ed.): *The Life and Letters of Charles Darwin* (2 vols., New York: Basic Books, 1959), vol. 2, p. 179 and *idem: More Letters of Charles Darwin* (2 vols., New York: Appleton, 1903), vol. 2, p. 137. Such glowing praise, however, was something of a habit for Darwin.
[18] Henry Thomas Buckle: *History of Civilization in England*, (1857: 2 vols., New York: reprint by Hearst's International Library, 1913), vol. 1, p. 3.
[19] *Ibid.*, pp. 199–200.
[20] *Ibid.*, p. 23.
[21] Charles Dickens: "A Few Facts about Matrimony", *Household Words*, 1850, **1**: 374: Dickens: *Hard Times* (1854; New York: Penguin Books, 1969), p. 300; George Eliot,

Daniel Deronda (1876; New York: Penguin Books, 1967), pp. 582–3. I owe the first and third of these references to I.B. Cohen and Lorraine Daston, respectively.

[22] James Clerk Maxwell: "Molecules", *Papers* (n. 2 supra), vol. 2, pp. 373–274.

[23] See Porter: *Statistical Thinking* (n. 4 supra), chapters 5, 8–9.

[24] Ludwig Boltzmann: "Über die mechanische Bedeutung des zweiten Hauptsatzes der Wärmetheorie" (1866) *Wissenschaftliche Abhandlungen* (3 vols., Leipzig: J.A. Barth, 1909), vol. 1, pp. 4–30; *idem.*: "Weitere Studien über das Wärmegleichgewicht unter Gasmolekülen" (1872), *ibid.*, pp. 316–317; *idem.*: "Der zweite Hauptsatz der mechanischen Wärmetheorie" (1886), *Populäre Schriften* (Leipzig: J.A. Barth, 1905), p. 34.

[25] Maxwell: "Illustrations of the Dynamical Theory of Gases", *Papers* (n. 2 supra), vol. 1, pp. 380 ff.

[26] John Herschel: "Quetelet on Probabilities", *Edinburgh Review*, 1850, **93**: 1–57, p. 23. Charles Gillispie first proposed that Herschel's essay exemplified an understanding of science conducive to the emergence of statistical theories like Maxwell's in his "Intellectual Factors in the Background of Analysis by Probabilities", pp. 431–453 of A.C. Crombie (ed.): *Scientific Change* (New York: Basic Books, 1963). The identity of these derivations was later pointed out by Stephen Brush; see his *The Kind of Motion We Call Heat*, 2 vols. (Amsterdam: North Holland Publishing Co., 1977).

[27] On these matters see my "A Statistical Survey of Gases: Maxwell's Social Physics", *Historical Studies in the Physical Sciences*, 1981, **12**: 77–116. This essay originated as a public lecture given at the University of Bielefeld, West Germany, while I was a research fellow at the Zentrum für interdisziplinäre Forschung (ZiF) there.

12. A CONVERSATION WITH HARVEY BROOKS* ON THE SOCIAL SCIENCES, THE NATURAL SCIENCES, AND PUBLIC POLICY – CONDUCTED BY I. BERNARD COHEN

IBC: Let me ask you, to begin with, when in your career as a scientist you remember becoming aware of problems involving the social sciences.

HB: I became aware of problems in science policy that involved what I would call social issues and social values very early in my career. But I did not think of them at that time as involving the social sciences directly. I really didn't become very conscious of this aspect of the question until perhaps the '60s, especially after 1965, when I became chairman of COSPUP, the Committee on Science and Public Policy of the National Academy of Sciences. This was also a period when we had considerable interaction with Fred Harris, who at that time was a Senator from Oklahoma and who had given prominence to such problems by proposing a National Social Science foundation. I became especially conscious of the social sciences, when I testified before the Harris subcommittee concerning that proposal. I recognized that the social sciences had something to offer in the policy process which had been neglected. I would say, therefore, that it was during the early to mid '60s that I first became explicitly conscious of the social sciences as such in their relevance to science policy.

IBC: That is more or less what I expected. It seems to be generally true for most natural scientists that relations with the social sciences didn't matter very much until the '60s. Probably the only place where this direct impingement did occur before the 1960s was in the National Science Foundation, which was giving research money to certain selected social sciences. And that is

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I. B. Cohen (ed.), The Natural Sciences and the Social Sciences, 363–398.

relevant to a very general question which I would like to ask before we turn to the National Science Foundation. I remember one of the newly appointed officials saying to me, "I've just discovered that we give a lot of money to the soft sciences." Have you any feeling about how, generally, scientists have regarded the social sciences and to what extent they have considered them to be "soft" as distinguished from "hard" sciences?

HB: This question goes back to the debate that took place between the Kilgore proponents and the Bush proponents when the National Science Foundation was being proposed.

IBC: By Senator Harley Kilgore, Democrat from West Virginia?

HB: Yes, he favored an agency which would, among other things, include the social sciences among those basic and applied sciences to be supported by the government. As you remember, Vannevar Bush had a more traditional "pure-science" approach. He wanted to have a foundation run by natural scientists that would be limited to basic research without any consideration of immediate practical outcome.

IBC: I remember that very well because I was on the research staff that prepared Bush's 1945 report, *Science: The Endless Frontier.*

HB: Really! I didn't know that.

IBC: Yes. Rupert Maclaurin of MIT organized a kind of "Secretariat" under the direction of Henry Guerlac who was then the historian of the Radiation Lab. One of the sections that John Edsall and I prepared was the comparison of research support in the United States and in England and France. I remember many discussions about the possible inclusion of the social sciences under the umbrella of a proposed national research foundation. I.I. Rabi was vehemently opposed to having the social sciences linked to the natural sciences. They work with opinion, he used to say, and not with fact. In the end, the Bush report did not envisage a role for the social sciences, but the enabling act left the door open by using the phrase "other sciences."

HB: In all these debates there was no consensus among natural scientists on whether the social sciences should be included, but the views of the natural scientists tended to reflect the views that existed in the population at large. Conservatives, those with more or less conservative political orientation, felt that the social sciences *were* soft. That is to say, the criteria for evidence and falsifiability of theories were not the same as in the natural sciences. Moreover, I think that politically the conservatives feared that the term "science" would become tarred with the political implications of the social sciences. Some conservatives equated the social sciences, especially sociology, with socialism.

In other words, many scientists feared exactly what did happen in the controversy over the MACOS project.

IBC: What was that? Was it "Man: A Course of Study"?

HB: Yes, that's right. This was a curriculum development project of the NSF which was to be similar to the already sponsored Project Physics, which developed an important and widely used new curriculum for physics in the secondary schools. There was great controversy, as you can imagine, over the content of MACOS. For instance, Senator Proxmire, who was a constant critic of NSF, raised issues about MACOS and John Conlon, a Congressman from Arizona held that MACOS tended to undermine basic family values. The MACOS project became so controversial that it had to be abandoned. There were many people in the science community who feared that this kind of controversy could affect the whole National Science Foundation and hurt support for the natural scientists. This was probably the greatest problem. The MACOS project was very controversial because it dealt with anthropological and social issues which themselves were subjects of great dispute and controversy, unlike any discussion about principles or theories of physics. This was the sort of issue that arose at the very beginning, when it was proposed that the social sciences be included in the NSF. Nevertheless, there was no consistent view of the social sciences on the part of natural scientists.

IBC: When you and others who come from the harder sciences, so-called, think of the social sciences, what subjects come to mind? Obviously, one of them is sociology.

HB: I would be more likely to think of sociology than I would of economics. I regard economics as being a kind of thermodynamics of the social sciences in the sense that economics starts with rather oversimplified models of reality and then proceeds to make rather rigorous deductions from those models without necessarily asserting that the models are rigorous but asserting rather that the deductions are rigorous. This procedure is analogous to the methods of thermodynamics in the field of physics. The scientist working in thermodynamics begins with some very broad postulates and then explores what can be deduced mathematically from them.

I separate economics somewhat from the rest of the social sciences. In some sense economics is the model of the social sciences that most corresponds to the physical sciences, particularly to physics. Turning from economics to the rest of the social sciences, I would say that most natural scientists would arrange the rest of the social sciences in a hierarchy of softness, ranging from quantitative sociology, survey research, and so on at one end, to political science at the other, although I realize, of course, that survey research cuts across sociology and political science. But in the softer social sciences there is a characteristic verbal tradition. Political science has been a verbal, descriptive science until very recently; sociology very early became part of a quantitative tradition.

I don't believe that there was any specific image of the social scientist in the mind of natural scientists. There was, however, one point of view in the natural sciences which I think it is important to underline. That is, there is a certain degree of skepticism among natural scientists concerning the models used in the social sciences. And this skepticism was based on the scientists' own experiences in the natural sciences. The difficulty of formulating models with which to work in the natural sciences was severe enough, even though one had a relatively small number of entities which were interchangeable – like electrons, protons, and so on. In the social sciences, however, every individual is in some sense different and although individuals have certain common properties there is a degree of arbitrariness in the classification scheme. Consequently, I think there was a skepticism among natural scientists whether social scientists could make models which bore sufficient resemblance to reality so that rigorous deductions would be valid. One could not be sure something important wasn't being left out of the model even in the natural sciences, so that one could not rely on the deductions, not because they weren't rigorous but because the model did not correspond sufficiently with the real physical world.

IBC: Let us explore this a moment. My studies show that most people from the natural sciences think primarily of sociology as . . .

HB: As a social science.

IBC: . . . as *the* social science. The discussions about the scientific character of the social sciences do not usually have much to do with history, political theory, archaeology, social anthropology or social psychology. I have always found it interesting that my acquaintances who are most skeptical about the alleged scientific character of the social sciences come from the physical sciences or mathematics. Would the same be true for those in the earth science? Or in the biological sciences? When you have discussions on these topics in the various organizations to which you belong, do people from different areas have different attitudes about the social sciences? For example, some natural scientists complain that in the social sciences prediction rates are not very high.

HB: Yes, that's true.

IBC: But earth scientists have great difficulty making predictions and yet are not rejected on this account. Earth scientists would not give success in prediction the importance which physicists would.

HB: That is very true. In fact, I have made that point explicitly in an article I wrote for *Minerva* in 1972 as a commentary on Alvin Weinberg's introduction of the concept of trans-science.

IBC: I remember that. Wasn't he Director of the Oak Ridge National Laboratory and a member of PSAC, the President's Science Advisory Committee?

HB: Yes. I objected somewhat to Al's use of the word "trans-science" because he seemed to categorize the social sciences as all in the field of trans-science. One of the problems I raised was that the social sciences had been taking Newtonian mechanics as their model, whereas meteorology was a much better model. I pointed to the work of Lorenz at MIT. He had done some fundamental work on the predictability of the weather, in which he showed rigorously from the equations of motion of the atmosphere that it is impossible in principle to predict the weather more than about fifteen days ahead, no matter how perfect the information on initial boundary conditions. This situation is connected with boundary layer phenomena because it is necessary to specify the boundary conditions, in effect, with infinite accuracy. In effect, infinitesimal changes in the initial conditions or boundary values of variables lead to large changes in predictions from the rigorous equations of motion. Thus arbitrarily small errors in the boundary values of the variables lead to finite differences in outcome of solutions of the equations. Another way of putting it is that the underlying assumption of the law of causality, that effects are proportional to causes, is violated, and can be mathematically inherent in apparently deterministic equations, such as the Navier-Stokes equations of hydrodynamics. Infinitesimally small causes can lead to finite effects mathematically. You can predict that tornadoes are likely to occur in a certain broad region at a certain time, but you can't predict exactly where they will occur or what their path will be. Thus the limit to predicting the weather is really determined by the boundary phenomena. This insight was perhaps the first inkling of the theory of chaos which has become so fashionable now. In fact, some of the writers on the theory of chaos refer back to that original paper of Lorenz in the '60s.

Yet the scientific mental model which was being used by most social scientists was still the idealized model of Newtonian mechanics. They were unaware of Lorenz's result or of many other models in physical sciences based on rigorous equations, which nevertheless could not make accurate predictions. After all, the hydrodynamic equations are rigorous, but they nevertheless exhibit boundary layer and turbulent phenomena that make the problem of predictability fundamentally different from what it is in a Newtonian mechanics of billiard balls or planets. In fact, one of the first people to do significant work in this area was George Carrier, who originally became famous through his mathematical analysis of boundary layer phenomena.

IBC: As I look through the literature I find that much of social science, even economics, still tries in some way to imitate Newtonian rational mechanics, sometimes with energy physics thrown in for good measure.

HB: Yes.

IBC: Even though much of even "classical" physics does not.

HB: Right.

IBC: There has also been a large infusion of energetics, particularly in economics.

HB: You asked a question from which I digressed somewhat: What was the attitude of biologists as compared with that of other scientists? I would say that the division of biologists into systematists and molecular biologists was so great that separate answers must be given for the two halves of biology. The molecular biologists, or, more broadly, the reductionist biologists, thought more like physicists, whereas the systematic evolutionary biologists thought somewhat more like social scientists and were inclined to be more sympathetic to the methodology of the social sciences.

IBC: Perhaps you might in this connection say something about the relative importance of physical scientists as opposed to biological scientists in all of the public bodies such as the National Science Foundation, the National Academy of Sciences, and the President's Science Advisory Committee.

HB: In the original constitution of PSAC, the President's Science Advisory Committee, there was, you might say, a compromise between representing the disciplines and assembling a group of people with a common language who could communicate with each other easily in their own code. In the early days of PSAC this adjustment was made very much in terms of the latter model, with the result that PSAC was dominated by physicists in the days of Eisenhower. There were various reasons for this situation. A major one was that most of the physicists on PSAC came out of a common experience in World War II, working on various kinds of military systems. Therefore they already had a considerable familiarity with the kinds of questions and issues that were first directed at PSAC by President Eisenhower, most of which arose out of the rivalries among the military services and the proponents of various military systems. Thus, it was very natural that in its early days PSAC was dominated by physical scientists. In fact, for some time these experts did not see the need of any other forms of expertise. This was not because they were blind to other forms of expertise, but rather because of the kinds of questions that were posed.

One of the first issues that faced PSAC that involved social considerations and had very high visibility was civil defense. A crisis over civil defense arose after Kennedy's confrontation with Khruschev over Berlin, and Kruschev's threat to abrogate unilaterally the four-power arrangement in Berlin. There was great internal pressure in the government for a major civil defense program. The Defense Department was given the responsibility for developing that program and they conceived it almost entirely in terms of physical and engineering aspects of civil defense: bomb shelters and so on. This became a very emotional subject. PSAC had formed its own panel on civil defense to monitor and advise on implementation of the program of the Department of Defense. It soon became apparent that while, as I have said, the program of the Department of Defense was almost exclusively focused on the physical and engineering aspects, the public and the press were much more concerned with the social, behavorial, and organizational aspects. With the blessing of PSAC, Jerrold Zacharias, who was very active in many aspects of science policy questions, put together a small informal panel in Cambridge that met on weekends to discuss the psychological and behavioral aspects of civil defense planning, and particularly the rising public reaction. I was a member of this panel, which included physicists on PSAC and three or four well-known psychoanalysts and psychiatrists. Some of the people involved were Ed Purcell the physicist from Harvard; Doug Bond, head of the department of psychiatry at Case Western Reserve; Grete Bibring, a well-known Cambridge psychoanalyst; Gardner Quarton, a professor of psychology at the University of Michigan; and Oliver Cope, the MGH surgeon, who had become greatly concerned over this issue, in part because of his experience in the Coconut Grove fire in Boston in the early 1940s. I don't remember whether or not Erik Erikson, the psychoanalyst, came to any of the meetings or whether a few of us talked with him privately about these matters. We had meetings every weekend during the height of the civil defense scare, with several of the people, like Gardner Quarton and Doug Bond, actually flying to Cambridge every weekend at their own expense. This group had considerable influence in helping Jerry Wiesner, the President's Science Advisor, to persuade President Kennedy that civil defense was not just a problem of physical hard science and that it was necessary to worry especially about what would happen after the first two weeks following a nuclear exchange. The protection of the population in the first week or so after an exchange of nuclear weapons was only a minor part of the challenge compared to the social disintegration and the psychological and social problems that would ensue during a later period. This was one of the earliest involvements of social scientists in the work of PSAC. There was no formal report or conclusions resulting from these meetings, but there was considerable mutual education. Informal advice from the panel to the Science Advisor, Jerry Wiesner, helped him to convince President Kennedy to hold the national program to more modest proportions than had originally been contemplated. This situation was one of the first, I think, in which PSAC really became conscious of the psychological and social aspects of such problems. Concomitantly it was obvious that the social sciences had something important to say about many other public policy issues involving the natural sciences or technology.

IBC: Your point seems extremely important because it bears on a very fundamental question, that of the social sciences generally in relation to policy questions.

HB: Yes. The influence of the social sciences on the formulation and implementation of public policy is not a new phenomenon. During the Great Depression of the 1930s there was a strong belief that the scientific study of social phenomena could make an important contribution to the solution of the national crisis which was then the focus of political attention. At the end of World War II, in the debates over the creation of Vannevar Bush's proposed National Research Foundation, one of the principal issues was whether there would be a role for the social sciences in the new policy for post-war government support of science. William F. Ogburn, the sociologist from the University of Chicago who specialized in the study of technological innovations, testified to Congress that inventions inevitably precipitate social change and social problems and that therefore a government which supports discovery and invention has a responsibility to support the social sciences in order to help foresee and deal with the problems resulting from the discoveries and inventions growing out of its support of the natural sciences and engineering.

With the heating up of the Cold War in the period from 1949 to the early 1960s, interest in the social sciences as underpinning for public policy waned, but it revived again in the early 1960s and early 1970s with the advent of the Great Society programs and rising public awareness and concern about the side-effects of technology.

In the late '70s and in the decade of the '80s, rising concern about the decline of the international competitiveness of the United States and the alienation of the American work force led to a new dimension in the application of the insights of social sciences to public policy. This interest was stimulated by growing awareness that Japanese success in international competition was as much due to innovations in work organization and the management of the work force as to advances in production hardware. With the advent of the Reagan administration in the 1980s, however, the policy relevance of the social sciences suffered a temporary eclipse as far as government was concerned, but it is now arriving in a new form with growing appreciation of the need for a better understanding of science and technology as social systems in order to provide a basis for the formulation of more coherent science policies. This understanding is important both in considering the allocation of national resources for scientific research and technological innovation and for understanding the role of scientific knowledge and scientists in the formulation of public policies with high technical content.

IBC: You have spoken already about a specific instance involving PSAC. Can you say anything further about the acceptance of social scientists on PSAC and about the consequent influence of the social sciences on the formulation of science policy?

HB: In order to answer your question fully I have to go back into a little history. Let me start with the years of the Eisenhower administration, when there was a Science Advisory Committee which since 1951 had functioned out of the Office of Defense Mobilization. In 1957, this was transformed into the President's Science Advisory Committee under the personal patronage of President Eisenhower and quickly became known as PSAC. The occasion for this transformation was the crisis of public confidence which followed the successful Soviet launch of Sputnik in October 1957. This even raised serious concern about science and science policy, and the great step taken by Russian scientists and engineers called for a new and more important role of natural scientists in the highest councils of government. The creation of PSAC became one of the most commented upon and studied events in the evolution of science policy after World War II, not only in the United States but elsewhere in the world.

In December 1957, two months after Sputnik, Eisenhower appointed James Killian as his Science Advisor. While it is true that Killian himself was not a scientist but had been trained as a humanist and became experienced as a generalist administrator, he had worked closely with scientists, understood their attitudes, and had become highly skilled in interpreting science to laymen. I think it is important for people to understand that the first (and in some ways the most successful) presidential science advisor was not a scientist at all. He had simply absorbed the scientific culture most of his life by being around MIT and working with scientists and engineers, so that he knew how they thought and had become familiar with their language and intellectual short-cuts. It is significant that, unlike Jerry Wiesner, he almost never briefed the President by himself. He also took along one or two members of PSAC to provide the technical part of the briefing, which he could then interpret to the President in the scientist's presence.

In its original form, as I have already mentioned, PSAC itself was a remarkably coherent and close-knit group, dominated by physicists, most of whom had worked together at the MIT Radiation Laboratory or in the Manhattan Project (or both) during World War II and had been deeply involved in government advisory activities and "summer studies" closely linked to major technological events of the Cold War. Moreover, in the original membership of PSAC social scientists were notable for their absence, a fact which was not an inadvertent oversight, but was, on the contrary, an issue much debated both within PSAC and by outside critics and observers of the Committee. Among these latter were the members of a European task force appointed by the Organization for Economic Cooperation and Development, OECD, to conduct reviews or assessment of national science policy in each of the OECD member countries. This task force, two of whose three members were social scientists, deplored the lack of representation of the social and "human" sciences not only in PSAC, but also in most high level U.S. government advisory committees and boards having to do with science policy.

IBC: I know you mentioned this earlier, but could you say a little bit more in detail about the two different concepts of what PSAC ought to be.

HB: One of the elements of the debate on this subject was the existence of two quite different concepts of the role of PSAC. One view was that PSAC should be broadly representative of the range of the intellectual interests and styles of thought in the entire U.S. scientific community, and that its function should be that of a kind of ambassador from the science community to the nation's top political leadership. The other view was that PSAC should be a close-knit group of wise scientific generalists who would act as interpreters of expert scientific analyses and judgments to the President and his immediate political advisors. Both the President and the Committee opted for this second model, but many people both in the scientific public and in the larger lay public outside the government tended to take the representational view and were therefore much more inclined to look at how representative PSAC was of the scientific constituency whose interests it was thought of as, at least partially, serving. In the generalist view of the Committee's role, however, disciplinary representation would be mainly through the some 300 panelists and consultants that were tasked by PSAC or the Science Advisor to look into specific technical problems on an *ad hoc* basis for the President. PSAC could also look to the numerous committees and boards of the semi-private, semi-public National Research Council – represented on PSAC itself by its chairman, the president of the National Academy of Sciences. The some 4,000 experts serving on the National Research Council committees covered the full spectrum of the natural sciences, engineering, the social sciences, and the learned professions.

IBC: Was there any notable difference in the kind of problem addressed in the early days of PSAC and the early 1960s that might have some bearing on the involvement of social scientists?

HB: In the early days of PSAC, when it was most relied on by the President, most of the problems addressed to it arose out of military technology and involved adjudication among conflicting claims and technological proposals emanating from the three military services. These conflicts could not be resolved except at the Presidential level. The problems were highly technical, but of a type which experienced physicists could quickly learn about. PSAC, as I said before, tended to be a closely-knit club whose members spoke to each other in a condensed code that all could understand. In fact, most members, even when they came from different research disciplines, possessed a more or less common technical culture derived from similar training and exposure to some common experience, mostly with military technology. Injecting even other physicists or engineers into this culture was difficult, and indeed when PSAC did attempt to broaden its base of membership, it found that some recruits dropped out after a few meetings, reluctant to pursue the attempt to master the arcane language. To have injected social scientists into this culture at this early stage in the evolution of the committee would probably not have worked; at least that is what both sides felt at the time.

Later in the 1960s, as PSAC's agenda of problems expanded outside the national security area, a few social scientists did join PSAC – first Herbert Simon in 1968 and two years later James Coleman. Herb Simon was not appointed to PSAC as an economist but as an artificial intelligence specialist. Coleman was a sociologist. Both Simon and Coleman were strongly quantitative in their interests and had had extensive interdisciplinary experience, which made it much easier for them to penetrate the PSAC culture.

But, by and large, PSAC was very wary of the danger of having only one representative of a discipline widely different from their own. They were fearful of becoming too dependent on a single expert and not being in a position to judge among a diversity of views in a field in which they were not themselves at home. If they had to have two of every discipline, then – like Noah's Ark, – PSAC would become hopelessly unwieldy.

IBC: Could you say something about the actual day-to-day work of PSAC? How much consisted of research and developing arguments as opposed to soliciting and judging opinions from various different experts?

HB: Much of PSAC's work actually consisted of listening to arguments among experts rather distant from their own fields of specialization. In fact, they frequently acted as a sort of report review committee for draft reports of their specialized panels before they were forwarded to the President or made public. In this role they thus served as scientific generalists rather than as highly specialized experts in their own right, although they usually became quite expert in short order in the problems which they worked on. They were sophisticated enough and sufficiently familiar with scientific reasoning to ask naive but penetrating questions that forced the experts to examine their implicit assumptions and possibly revise insufficiently supported conclusions. This critical function of PSAC was never well understood by the outside world or the technical community at large.

IBC: There were, however, some essays of PSAC into the social sciences as I remember, weren't there?

HB: During the late 60s. Eventually, PSAC did branch out into the social sciences, issuing among other papers an excellent report on Youth prepared by a panel chaired by James Coleman, who, as I have said, was a member of PSAC at the time.

IBC: Was the report the one called *Youth: Transition to Adulthood* which came out in 1973?

HB: Yes. There were also several reports on issues of government support for the behavioral sciences.

IBC: One of the ones that attracted the most attention, of course, was the 1962 *Strengthening of the Behavioral Sciences*.

HB: Yes, but this was only one of several reports on aspects of support for the social sciences. For instance, there was a study of the problems of privacy and research and another on privacy and behavioral research. There were also numerous reports on education. I have already mentioned that one of the earliest involvements of social scientists in the work of PSAC never resulted in a public report. This was, of course, the work of the Civil Defense Panel, as well as the activities of the informal Cambridge group in relation to civil defense and the threat of nuclear warfare.

IBC: I believe we have said enough about PSAC. Let me turn now to the National Academy of Sciences. Would you say that the NAS furnishes another striking instance of how the social scientists as a group were only later included among the scientists and, as a further step, an example of how the social sciences were included among the sciences?

HB: The two original learned academies of the American Colonies – the American Philosophical Society of Philadelphia founded in

1743 and the American Academy of Arts and Sciences of Boston founded in 1780 – embraced all areas of learning, including the natural sciences, humanities, the arts, the learned professions, public affairs, and subjects which we would call today the social sciences, although the latter were little recognized then. This expansiveness was more in the tradition of continental Europe. When the U.S. National Academy of Sciences, or NAS, was chartered by Congress in 1863, it followed the British pattern of electing only natural scientists and a few engineers, and it continued in this pattern for its first century of existence.

IBC: This is a very interesting point. Of course there are always some exceptions. For instance, on the continent the French Academy of Sciences has been even more rigorously restricted to natural science and mathematics than the British Royal Society, but the Berlin Academy from the start included humanists and social scientists among its members along with natural scientists and mathematicians. As a matter of fact, even though the NAS was originally restricted to natural scientists, the class of natural history did include ethnology and there were almost always at least a few members who could be considered social scientists.

HB: During World War I the National Research Council, or NRC, was created for the purpose of strengthening the National Academy's advisory function to government, which has been part of its charter from its beginning, but which it has never extensively exercised. The NRC permitted the recruitment of non-Academy members to its committees, while the prestige of the NAS membership served to legitimize the advisory work of the NRC, which often dealt more with applied science and engineering than with the pure science which qualified most of the elected members. But relatively little of the NRC's work dealt with the social sciences or their application in this early period.

IBC: I have a few notes that I have put together on this subject because it is significant for our discussion. In 1865 William Dwight Whitney, the sanscritist, was a member of the NAS. In fact, he was the only member in the section called ethnology. By 1866, however, the name of this section had been changed to ethnology and philology. It also contained a second member, George Perkins Marsh, whose scholarly work certainly fit both categories. In 1899 the constitution was amended to establish six "standing committees" including one called anthropology. In 1905, as the annual report relates, the president of NAS appointed a committee which included William James "to report to the council on the relations of the Academy to the philosophical, economic, historical, and philological sciences." In that period, not only James but also Charles Sanders Peirce, the philosopher and mathematician, and Josiah Royce, the philosopher and psychologist, were members. In 1910 John Dewey was elected, and in 1911 he became a member of the new committee called anthropology and psychology. In 1965 the Academy formally recognized a class of "biological and behavioral sciences," which in 1971 was divided into two classes, including one called behavioral and social sciences. In 1975 this class had four sections: anthropology, psychology, social and political sciences, and economic sciences. Even historians of science were recognized when two prominent members of my profession, Martin J. Klein and Otto E. Neugebauer, were elected in 1977; they were classified respectively under physics and astronomy. Klein had started out as a solid state physicist and would have merited election for that earlier scientific work, but Neugebauer had never been a practicing astronomer. In 1977, when Robert Merton was chairman of political and social sciences, Kenneth Arrow was chairman of economic sciences. They had been able to change their membership category in the '70s, but when they were elected in 1968 Merton had been listed under anthropology and Arrow in applied physical and mathematical sciences. Arrow was elected as an applied mathematician and his work in this area was distinguished enough to earn him a place in the NAS but Merton was certainly not an anthropologist in the narrow professional sense and his election was based on his studies of the sociology of science.

HB: I am glad you mentioned those examples. The case of Merton and Arrow seems to me to symbolize two aspects of the history of the NAS which would repay further study. The NAS appears to have admitted representatives of disciplines other than those in the accepted categories of the natural and mathematical sciences at a time when their own fields were not yet formally recognized as sciences. These individuals, including social scientists, were included in one of the classes of members according to the prevailing system but might later change to a more appropriate class if it was introduced. It would seem, therefore, that there has been a lag between a general recognition of work as scientific – a recognition shown by election of a particular member – and a reorganization of the Academy in official recognition of the scientific character of a discipline.

More generally, it can be said that in the 20th century the natural science qualification was broadened to include a few subjects such as psychology (mostly experimental), archaeology, and anthropology (mostly physical). By the late 1960s a few of the most prestigious and quantitatively minded economists had been elected to membership. By the early 1970s new sections were added on social and political science, and thereafter the number of social scientists in the NAS grew slowly, though not in proportion to their population in the American scholarly community.

IBC: A phenomenon which seems curious to me and to many others is that certain of the social sciences such as anthropology and archaeology, the latter of which includes people engaged in biblical studies, were the first to be considered legitimate parts of the Academy of Sciences. What are the reasons that they were accepted with such relative ease?

HB: I don't know whether I can give you an authoritative answer. It is certainly true, though, that the rise of the Great Society programs increased the general visibility of social scientists as a group and helped to arouse an interest in the social sciences from a policy point of view. At the same time, attitudes towards the legitimacy of the social sciences are revealed by the fact that the elections to the Academy were highly selective, choosing those social scientists whose work was regarded as being very objective. One thinks especially of an anthropologist-archaeologist such as Bob Adams or Gordon Willey, an archaeologist elected in 1960. In general, the archaeologists were considered to be dealing with physical artifacts, and the physical anthropologists were admitted to the Academy long before social and cultural anthropologists. But even social and cultural anthropology had a legitimacy in the Academy that other social sciences did not, largely, I suspect, because they studied societies other than our own. The anthropologists who were elected to the Academy, for the most part, were those who dealt with primitive cultures. There was a concept of objectivity that seemed applicable to their work, which posited an observer of an external world – a distinction that becomes fuzzier and fuzzier as one approaches closer and closer to one's own society. Hence, the parts of the social sciences that first became legitimate were those in which the subjects or objects of study were clearly separate from our own culture. This category clearly includes both anthropology and archaeology.

IBC: I am interested in this attitude because I find that, as in France and England, in the United States the sciences in the National Academy have tended to be considered largely natural sciences in a narrow sense. The social sciences are not really a vital part. And, as we have mentioned, there is no place for humanities. And yet in the German Academy and in the Italian and Russian Academies, unlike the French Academy and the British Royal Society, there is a place for the social sciences and the humanities.

HB: This topic has another important aspect because even engineering has been very poorly represented in both the National Academy and the Royal Society, relative to their representation in the professional population.

IBC: One of the reasons may have been that the pure natural sciences in the United States at that time were struggling for recognition and were badly organized and badly supported. They didn't want to dilute their efforts by an admixture with applied sciences or engineering.

HB: Yes, that seems right.

IBC: It was hoped that the formation of a National Academy would eventually serve to promote recognition of the natural sciences. But now let us turn to other aspects of the National Academy of Sciences and to its Committee on Science and Public Policy in particular, developing the theme as you have begun to do, but also considering the way in which you personally saw that you might accomplish something for the advancement of the social sciences.

HB: Yes.

IBC: You succeeded George Kistiakowsky as chairman of the Committee on Science and Public Policy, or COSPUP, didn't you?

HB: Yes. On July first, 1965.

IBC: I take it you were the chief instigator of the famous report on the outlook and needs of the behavioral and social sciences?

HB: Herbert Simon and I were the chief instigators. Now Herb Simon was elected to the Academy before there was a class of the social sciences in the Academy, and he was elected as a psychologist because psychology did have legitimacy among the natural sciences. Consequently, Herb Simon stands out as a social scientist elected to the Academy.

IBC: Right. In 1967, when Herbert Simon was elected, there was no class or section with the designation of social science. There was a class called Biological and Behavioral Sciences which, as we have mentioned earlier, had been established in 1965, but the only sections in that class that were properly behavioral sciences were anthropology and psychology. It was not until 1971 that the class called Behavioral and Social Sciences was formally inaugurated and not until the following year that the section designated as Social, Economic, and Political Sciences was introduced. In the next year, 1972, Herbert Simon left the section of psychology for the new section, and in 1975, when this section was itself divided into two, he joined the section on social and political sciences.

HB: These details illustrate the same developments which we have seen in the cases of Robert Merton and Kenneth Arrow. But the reason why I bring up Herbert Simon's affiliation is that the Committee on Science and Public Policy, or COSPUP, was originally established with one representative from each of the sections of the Academy; at the time there were only fourteen sections, whereas now there are twenty-five. Every member of COSPUP had to be a member of a section of the Academy, so Herb Simon's election to the Academy made it possible for him to serve on COSPUP when I was chairman.

Actually, the origin of the social science report was a conspiracy between me and Herb Simon that began even before he became a member of COSPUP. Since he was chairman of the board of directors of the Social Science Research Council in 1965 and COSPUP had been doing studies of sciences such as physics and chemistry, we thought it would be a wonderful idea for the SSRC and COSPUP to cooperate in doing a study of the social sciences. Ernest R. Hilgard was asked to chair this study, and he and Herb and I and others obtained funds from the National Institutes of Health, the National Institute of Mental Health, the National Science Foundation, and the Russell Sage Foundation to do the Behavioral and Social Sciences report. We defined the subject very broadly including, for example, booklets on history and geography as well as on the more conventional social sciences. That was really the origin of the so-called BASS report, which had the official title of *The Behavioral and Social Sciences: Outlook and Needs*. It was presented as a report by the Behavioral and Social Sciences Survey Committee under the auspices of the Committee on Science and Public Policy of the National Academy of Sciences and the Committee on Problems and Policy of the Social Science Research Council. This report was published in 1969.

IBC: Do you have any feeling about what the general attitude of the Academy members was or what any particular attitudes were towards this study?

HB: COSPUP was very supportive of the idea of having a report on the behavioral and social sciences. I don't remember any skepticism at all. Although the report was produced before the Academy was reorganized to include the class of behavioral and social sciences, I have no recollection of any opposition. Rather, as I have said, everybody was very supportive of the idea of having that kind of joint study.

IBC: Did you ever receive detailed information about the effect or influence of the report?

HB: It is rather hard to quantify that. In fact, the same problem has always applied to all the COSPUP studies, and I was asked that kind of question many times when I was chairman. The recommendations of priorities, when they occurred, had almost no effect. But they did establish the agenda. Thus, all the debates that occurred were about the categories that were developed in the COSPUP reports, including those on the social sciences. In other words, what the COSPUP reports did was to develop a rather carefully selected menu of opportunities by setting forth what the content of the various subfields was and what their implications might be for practical societal problems. And that very much set the terms of debate in the political sphere about priorities in science across the board, no more and no less in the social sciences than in the natural sciences. However, it was only the priorities *within* broad fields which were debated, not between broad fields – such as between, say, physics and biology.

IBC: Who was the chief force behind the founding of COSPUP? Was it George Kistiakowsky?

HB: Yes. The origin of COSPUP was very specific. When Kistie was Science Advisor to President Eisenhower in 1959–60, one of the last things that was done before he resigned from this post at the end of the Eisenhower administration was congressional authorization of initial funding for SLAC, the Stanford linear accelerator project for research in nuclear physics, of which Pief Panofsky was director.

IBC: That was Wolfgang K.H. Panofsky, professor of physics at Stanford and a member of the President's Science Advisory Committee.

HB: Yes. Although Kistie supported the proposal, he was very much troubled by the fact that this particular project had been elevated to the Presidential level and that one of the principal protagonists was a member of PSAC. Kistie was concerned that there was no neutral body to which appeal could be made for an evaluation of this kind of proposal. He believed that the National Academy would be sufficiently removed from such specific issues that it could serve the political system in helping to make this kind of choice. That is, he held it was improper to have so many major issues dependent upon PSAC. Therefore, as soon as he stepped down from his position as Science Advisor, which he did in January 1961, he went to Detlev Bronk, then president of the National Academy of Sciences, and proposed the idea of setting up a special committee that would be unlike the National Research Council, where, as we mentioned earlier, members did not have to be members of the Academy. Bronk agreed. As constituted, COSPUP was not part of the National Research Council but was a committee composed entirely of Academy members and reporting directly to the president and council of the Academy. Its function, as envisioned by Kistie, was primarily to assess the health and opportunities of the various scientific disciplines and to do this in a public fashion which would enable its findings to enter the political debate. That was really the origin.

Very soon, however, COSPUP expanded into other fields such as those concerned with population and various areas of public policy. The first public policy debate of this sort involved the McElroy report. William McElroy chaired a study of population which had a great deal of influence and resulted in the first federal support of family planning programs. This was called *The Growth of World Population* and appeared in 1963. From the start, therefore, COSPUP dealt with public policy problems involving science as well as with the health and opportunities of the various scientific disciplines and of sciences in general.

One of the first reports, not on the disciplines but on the general support of science, was the report on basic research and national goals which appeared in 1965.

IBC: Is that the one called *Basic Research and National Goals*?

HB: Yes. It was produced at the request of the House Committee on Science and Astronautics on the initiative of Congressman Daddario of Connecticut, who chaired the subcommittee on Science, Research, and development. When the report was finally issued in March, I was about to become chairman of COSPUP, but the report was started under the chairmanship of Kistiakowsky, who was also chairman of the ad hoc panel on basic research and national goals which prepared the report and of which I was a member. In fact, I helped write quite a bit of that report. This document was of special interest because it was prepared in response to two large questions about supporting research which were posed to the National Academy of Sciences by the Congress. Congress actually awarded a contract to the Academy to produce the report.

IBC: Does COSPUP exist still in some form?

HB: Yes, but it has been transformed, although it has had a continuous existence since the beginning. After the Engineering Academy was founded in 1964, a similar group, the Committee on Public Engineering Policy, was established by the new Academy. This happened in 1966, and the committee received the acronym COPEP. In 1981 COSPUP was reorganized as a joint committee of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine and was renamed the Committee on Science, Engineering, and Public Policy, or COSEPUP. Originally, every section of the Academy of Sciences was represented on COSPUP, but as the Academy expanded the number of sections the membership of COSPUP no longer was based primarily on representation from all the sections. In addition, since the members of COSEPUP are members of either the Academy of Engineering, the Institute of Medicine, or the Academy of Sciences, the committee is no longer confined to the Academy of Sciences as it was in the early '60s, when there was no Institute of Medicine or Academy of Engineering. Hence, the answer to your question is that COSPUP does in fact still exist, as COSEPUP, and is in fact very active.

IBC: But it does not still issue the older style of report?

HB: True, it stopped issuing reports on the disciplines.

IBC: Are the current activities and reports devoted to general or to specific scientific issues?

HB: At the beginning of Frank Press's regime, when Jay Keyworth was the Science Advisor, COSEPUP produced a series of ad hoc reports on what I would call new opportunities in science or opportunities for new initiatives in science. These were quite good reports.

IBC: We can keep in mind that Frank Press became president of the National Academy of Sciences on July 1, 1981, and that George A. Keyworth II was President Reagan's first Science Advisor, serving in that capacity and as Director of the Office of Science and Technology Policy, from 1981 to 1985.

HB: Yes, and that is relevant to another issue which concerned COSEPUP and had its origins in 1976. You remember that PSAC, the President's Science Advisory Committee, was abolished in 1973 and that in 1976 a law was passed by Congress to create the Office of Science and Technology Policy and the post of Director of the Office of Science and Technology Policy and Science Advisor to the President. As a consequence, a new form of PSAC was recreated in 1976, largely under the initiative of Nelson Rockefeller, who was Vice President to President Ford. Guy Stever – that is H. Guyford Stever, who was Director of the National Science Foundation between 1972 and 1976 – became the first new Science Advisor. After Ford was defeated by Jimmy Carter in 1976, there was quite a long hiatus during which Carter did nothing about appointing a Science Advisor. Finally, however, Frank Press was appointed.

It must be kept in mind that the '76 law, over the opposition of almost all past Science Advisors, mandated a rather elaborate system of reports. One of these was to be a five-year outlook for American science. Another was to be an annual report. The President was supposed to draw upon information prepared by the director of the Office of Science and Technology Policy in order to transmit a report to Congress containing recommendations for legislation and policies. Neither Frank Press nor Jimmy Carter particularly wanted these responsibilities, and there was much discussion. What finally happened was that in 1977 the responsibility for both the five-year outlook and the annual report was delegated to the National Science Foundation. In 1978 the National Science Foundation contracted with the National Academy of Sciences to do a report which would constitute a large segment of the five-year outlook. By 1982 the responsibility for developing the NAS report had been given to COSEPUP.

IBC: Was the five-year outlook published by the National Science Foundation or by the National Academy of Science?

HB: By both. For example, the first five-year outlook done by the National Academy of Sciences appeared as a separate book published by W.H. Freeman in collaboration with the NAS in 1979.

IBC: That was the one called *Science and Technology: A Five-year Outlook*?

HB: Yes. But that report was also published as a part of the NSF five-year outlook in 1980. The NSF publication was much larger, consisting of two volumes.

IBC: That was the publication entitled *The Five-Year Outlook: Problems, Opportunities and Constraints in Science and Technology*.

HB: Yes. The first volume was a topical synthesis prepared by the NSF, while the second volume comprised source materials divided into three sections, of which the first was the NAS report, the second contained reports submitted by selected government agencies, and the third consisted of papers written by individual specialists.

IBC: Was the second five-year outlook prepared and presented in the same way?

HB: Very much so. In this case, the NSF document consisted of three volumes, which appeared in 1982.

IBC: That one was called *The Five-Year Outlook on Science and Technology, 1981*.

HB: This time the NSF publication contained one volume of synthesis and generalization prepared by the NSF on the basis of the source materials published in the two "source volumes." I have these volumes right here. The second of these source volumes had three sections, of which the first was a report from the American Association for the Advancement of Science called *Policy Outlook: Science, Technology, and the Issues of the Eighties.* This was also separately published in 1982 by the Westview Press with the elements of the titled reversed to read *Science, Technology, and the Issues of the Eighties: Policy Outlook* and credited to Albert H. Teich and Ray Thornton as editors for the American Association for the Advancement of Science.

IBC: Wasn't there also a contribution from the Social Science Research Council?

HB: Yes, the second section of the second volume of source materials was a report from the SSRC entitled *The Five-Year Outlook for Science and Technology: Social and Behavioral Sciences.* I don't know whether this was published separately by the Social Science Research Council.

IBC: Brief descriptions of the essays did appear in the Social Science Research Council's *Items* and an announcement of the NSF publication also appeared in *Items.*

HB: The third section of the second NSF source volume contained "perspectives" presented by federal agencies. But what is of special interest to us now is this first volume, which consisted of the report submitted by the National Academy of Sciences. This was entitled *Outlook for Science and Technology: The Next Five Years* and was issued as *A Report from the National Research Council.* This was also published separately in 1982 by W.H. Freeman in collaboration with the National Academy of Sciences.

IBC: What about later outlooks?

HB: The third and fourth were prepared by the Committee on Science, Engineering, and Public Policy and were published in 1983 and in 1985 or 1986. These were very slim volumes.

IBC: Let's turn next to the National Board and the National Science Foundation. I believe that in many ways this may prove to be a most interesting topic because there has been a rise and fall of the social sciences, so to speak.

Let's go back to when President Truman signed legislation creating the National Science Foundation along the basic-scientific-research lines advocated by Vannevar Bush in his 1945 report, *Science; The Endless Frontier*. In addition to the Director, the law empowered a twenty-four member National Science Board to administer the Foundation. Most of those chosen by Truman were scientists, scientists turned administrators, or industrialists and leaders of public affairs whose work related to science. Only two represented the social sciences. When did you become a member?

HB: I became a member of the National Science Board in 1962 and retired in 1974. Thus I had two full, legally permissible, six-year terms, on the Board. I think Phil Handler and I were the only two people who actually stayed on the Board for the specified maximum "twelve years." What questions should I address?

IBC: For this conversation the primary question is the role of the social sciences in the NSF. These years when you were on the National Science Board are really the crucial ones.

HB: Yes, I agree.

IBC: In 1960, the National Science Foundation established a separate Division of the Social Sciences. Ten years earlier, at the beginning of the Foundation, research support focused almost exclusively on the physical, mathematical, engineering, biological and non-clinical medical sciences. For most of the fifties, a token support of the social sciences was introduced by giving a broad interpretation of the term "and other sciences" in the enabling act. In those days the social sciences were funded under "psychobiology," "anthropology and related sciences," and "sociophysical sciences." It was not until 1958 that the Foundation formally established an Office of Social Sciences to support basic research in the anthropological, economic, and sociological sciences as well as in the history and philosophy of science. Two years later, the Office of Social Sciences was reconstituted as the Division of Social Sciences. The latter continued to function as a separate division of the NSF from 1960 to 1975.

Hank Riecken, a social psychologist, served as Head of the Office of Social Sciences. With the reorganization, he became Assistant Director for the Division of Social Sciences and continued in that capacity until 1964 when he was named Associate Director for Education.

HB: That seems right. The important thing is that social sciences had always been to some degree supported by the NSF.

IBC: Yes, but on a very limited scale.

HB: During the Great Society period, however, there was a tremendous increase in the support of the social sciences, about four fold within a few years.

IBC: That's right. Now my notes show that in 1967, the Democratic Senator from Oklahoma, Fred Harris, proposed the creation of a national social science foundation modeled on the National Science Foundation. Not only was there little support in Congress for a separate social science foundation, but social scientists themselves testified at Congressional hearings, expressing serious misgivings. Therefore, instead of authorizing a separate social science foundation, there was the 1968 Daddario-Kennedy Amendment, which modified the original National Science Foundation charter. The new legislation permitted the Foundation to sponsor applied research and designated the social sciences as a field eligible for support. How were these developments viewed by the National Science Board?

HB: I don't remember very much debate in the National Science Board about the desirability of encouraging and using the social sciences. But I do recall very well the effect of the proposal of Senator Fred Harris. You're right about the social scientists; most of them were opposed to the Harris plan. Pendleton Herring, President of the Social Science Research Council, and many others did not want to have a second foundation. They preferred to increase the status of the social sciences in the NSF and in other branches of government. I don't remember any opposition, any real resistance on the Board to the social sciences, although there was some fear that they could become overly politicized. This was always a concern on the part of the Board and of the Director of NSF, a sense that the social sciences always run the risk of producing a political sensation. Thus the Board and the NSF always tried to avoid subjects and projects that had a very strong political flavor. Even in the area of political science, there was an inclination towards opinion polling and survey research, work that was empirical and quantitative, but did not support any particular political viewpoint.

IBC: You are quite right. There was a history of support for the social sciences in the National Science Foundation almost from the start. I was very active in the Foundation in the pre-Riecken and early Riecken days. That was when NSF funded the so-called "sociophysical sciences" through the Division of Mathematical, Physical, and Engineering Sciences. The subjects we supported were primarily anthropology and archaeology, mathematical economics, sociology, and the history and philosophy of science.

HB: Do you remember when that was?

IBC: In the mid-fifties, under the benevolent guidance of Ray Seeger, a physicist. The head of this section was Harry Alpert, a sociologist, who later became Dean of the Graduate School of the University of Oregon. At that time, as I recollect, some nonscientists were added to the National Science Board.

HB: I remember Father Ted Hesburgh, President of Notre Dame. But I don't know whether he was selected for his academic expertise or as a general advisor.

IBC: Of course, he would always make an important contribution whatever his official role.

HB: There were people like him on the Board who might be called social scientists, but I don't believe they were really chosen as representatives of the social sciences.

IBC: Did you personally have any feeling about creating a separate division of National Science Foundation for the social sciences?

HB: I was very supportive of the idea of the social sciences being better represented in the National Science Foundation.

IBC: Did your time on the National Science Board overlap the demise of the social science division?

HB: No. That occurred a year after I left the Board. The social sciences were reorganized into the Division of Biological, Behavioral, and Social Sciences in 1975.

IBC: To me perhaps the most interesting aspect of that whole story is that there was never any great movement either to prevent the decline of the social sciences in NSF or, afterwards, to restore a separate division.

HB: Yes. Everybody seemed to accept it.

IBC: Let me turn to another point. There has long been a great discussion, as you know, about whether the social sciences are sciences. I have recently been examining an enormous literature on this topic, much of it written by sociologists, particularly on the question of the qualifications that define a science. Then, when this question has been answered, the authors examine whether the social sciences – and sociology in particular – can meet the qualifications. The social scientists almost all seem to agree that sociology is a science, while natural scientists, for the most part, disagree. What is your view?

HB: I have always felt that the best criterion for a science is that it must be a body of knowledge in which the theories are falsifiable. I think that falsifiability, as it is defined by Karl Popper, while it should not be pushed too far, is an important characteristic that distinguishes the sciences from other activities. For instance, I don't consider the kind of sociology that Dan Bell does, much of which I greatly admire, to be "real science" because I cannot figure out a way of proving it wrong. I have done a great deal of work in close alliance with the social sciences myself, and I must admit that I always become very uncomfortable with the fact that in much of what is written, there is frequently little consideration of alternative hypotheses. This is the feature which leads me to say that the basic criterion for science should be the falsifiability of theory. In other words, the theories and the concepts in a science ought to have the possibility of being proved or disproved by evidence and analysis.

IBC: Or at least framed in such a way.

HB: Right. Framed in such a way that they can be disproved. One can push that too far, of course.

IBC: That is right. A good example is the status of biological evolution. Popper himself did a certain amount of wavering with regard to evolution. He long believed that since evolution was not falsifiable, it could not be a scientific theory. Later he changed his mind.

HB: Of course, as Bernie Davis frequently points out, evolution never became a completely solid theory until molecular biology was developed, because molecular biology provided the first real microscopic evidence of the connectedness of all life. Evolution as formulated by Darwin is somewhat analogous to thermodynamics in physics. Darwin, it seems to me, did not penetrate far enough into fundamental mechanisms that one could be absolutely sure that things could not be explained by alternative hypotheses. That is the trouble, by the way, with Popper and the Popperian criterion. Even though Popper's criterion is fundamentally correct, if one adopted it with respect to every theory, one would never make any progress.

IBC: I agree. Let us now return to the specific subject of this book: the relations between the social sciences and the natural sciences. We have thus far covered a number of aspects of this general topic. But there still remain some aspects of the public policy issue that I want to explore with you. They can be posed in relation to the famous Coleman Report of 1966. This report seems to be remarkable not only because it was a social science report that directly affected policy but also because it was actually mandated by the Congress. I believe it may even have been the first report in the social sciences ever to be mandated directly by Congress. Usually, such reports have been produced at the request of some agency in the executive branch of government.

HB: I believe that is correct.

IBC: I have just been rereading the Coleman report and also Coleman's most recent book, *Foundations of Social Theory*, published by Harvard University Press. To me what is most interesting about this huge book is that it contains very little discussion of social progress, social problems, or social policy. It looks like a book on statistical thermodynamics. This aspect is really quite extraordinary. If one examines comparable works of other sociologists – Will Ogburn's, to take a classic example, or the Hoover Report – one finds almost always a dual concern: making a social-science analysis and also an attempt to influence policy and improve the state of society, or at least to call attention to some of society's problems. Clearly there is a difference here between sociology and the physical sciences, where the primary aim is to know or to understand nature or even to control nature, but not to improve it.

HB: That would be engineering, an application of knowledge.

IBC: Good! Let's make a distinction between physics and engineering. This leads me to a fundamental question: Do you think there is a lack of this distinction in social science, notably in sociology? What I want to explore is whether the confusion between social "science" and social "engineering" may have been a factor in considerations of whether social sciences are sciences? Has it ever been a factor in the use or non-use of the social sciences in questions of public policy? In other words, do you believe that the complaints of social scientists about the very small role of the social sciences in relation to policy questions is related to a failure of social scientists to keep separate questions of knowledge and questions of advocacy? Is there any concern on the part of natural scientists that perhaps sociologists may be more concerned with the word "social" than with the word "science"?

HB: It depends. I don't think the scientific community is monolithic in this respect. It seems to me that there are two kinds of social scientist, both of which have valid claims. Jim Coleman represents one sort of extreme. From what you say about his book, he seems to have moved even further in that direction, namely, believing that it is the job of the social scientists to describe reality and let the chips fall where they may. But there are surely scientists who find that the social sciences are more like engineering. In fact there is sometimes almost an equating of social science with social engineering.

I remember one incident which made me very uncomfortable when I was on the National Science Board, during the heyday of the popularity of the social sciences in Congress. Some of the members of the Board appeared before the Congress and testified almost to the effect that the knowledge being gained in the social sciences that the NSF was supporting would enable us to engineer society. I am surprised that this attitude did not provoke a stronger negative reaction from the Congressmen. They have been said, after all, to consider social engineering their exclusive province. But I remember this event distinctly because I was there when the testimony was given. It made me very uncomfortable because it bordered on the claim that the social sciences would enable us to manipulate society. Certainly during the 1960s, especially the late 1960s, there was a period of hubris in the social sciences, when quite a few people believed that the social sciences were really going to make it possible to engineer society. And while most good social scientists denied any such claim, there was much rhetoric that suggested otherwise.

IBC: I think that this is a very important point. As I mentioned in my introduction to this book, the very first issue of the *American Sociological Review* stressed a dual aim: that it would report progress in the science of understanding society but also deal with social problems. And this social ethics or social engineering feature has grown up, side by side, with the advancing knowledge of social phenomenon. There is an ambivalence of aims here which may be similar to the situation which obtains, to some degree, in medicine.

HB: That is right.

IBC: You once told me that you believe that probably the most important of the social science reports that have affected American public policy was the Gunnar Myrdal report on *The American Dilemma: The Negro Problem and Modern Democracy.*

HB: Right.

IBC: Could you explain? I agree with you, but I find on reexamination of this report that it was not issued as a scientific study. There are no claims that it presents the latest findings of science. It became – to a large degree – a statement of Myrdal's personal convictions, even though he drew extensively on the research done over many years by a committee of research social scientists. The case was amply documented, to be sure, but it reads somewhat as an extended moral tract, as an indictment of what society had done to one particular class and its members. The Coleman Report, on the other hand, was based on a scientific investigation and issued as a scientific report. The conclusions were statistical rather than moral. The volume has the appearance of an engineering study and is not at all like a moral tract. Have you any feelings about the relative impact of the two reports? In the end, in terms of net effect on public policy, was there much difference? In other words, what is the effect of the "science" content of social science on questions of policy?

HB: That is a very difficult but important question. The Coleman Report certainly had a great impact at the time, although in retrospect some people feel that some aspects of that impact were perhaps not legitimate. Was it not the Coleman Report which indicated that there had been very little benefit from Head Start, for example? But the general tone of the Coleman Report, or the general conclusion which most people drew from the Coleman Report was that the evidence for the effectiveness of intervention in the educational systems was not very persuasive. For example, one of the things that Coleman found was that there was virtually no correlation between per-pupil expenditures in schools and academic performance.

There is, as you say, a tradition in the social sciences of both knowledge and programmed action. This reflection leads to a general consideration of the relationship between policy and science. One of the roles of science in policy is to project, as objectively as possible, the most probable outcomes of various alternative policies. There are some people who would say that this should be the only contribution of scientists to the policy process: not to recommend policies, but to predict, as well as possible from the data available, what the probable outcomes will be from various alternative policies. And this is the tradition in which Jim Coleman was operating. I think myself that we need both traditions, but people should be more candid in stating which tradition they are following.

I like to draw a parallel between the physical sciences and engineering and have often said that in the whole "science and society" field there are really two different traditions, both of which are very important and both of which have a place. One is the "science, technology, and society" tradition in which the student is primarily interested in the phenomenology of the interactions between science and technology and society. The other is the "science policy" tradition, which is more like engineering, and in which the student is more concerned with policy design. Both traditions, as I say, have a place, and they are obviously interrelated by the common effort to predict the consequences of various policies. This is also true of engineering. In engineering one uses science to predict the performance of various designs, and engineering cannot exist without science in this sense, the ability to predict the performance of various designs. It is impossible to have science policy without some ability to predict the consequences of various policies. I think, therefore, that both of these traditions have an important place in the system, and this is true whether one is talking about the natural sciences or the social sciences.

IBC: That is a very important point. I have on my shelf at the moment a continual sequence of books – there must be forty or so – which have come out, and still are coming out, all having the same lament: that applied social sciences is not as yet considered to be policy research, in the sense that it does not influence policy. There is concern that social science research or policy research is used only when the person who is determining the policy finds that the results of the research can support an already adopted position. There is thus a continuing uneasiness on the part of social scientists because they find that policy goals are not determined by their research. Would you like to say something about this?

HB: My comment on that must revert to the analogy about the relations between science and engineering. The fact is that science does not determine the design of artifacts. When a man is sent to the moon, that is not a scientific project, but it cannot be done without the ability to predict how the various artifacts that are built are going to perform. And if the artifacts are very large and complicated, it may not be possible simply to build and test them, but it will be necessary to infer their performance from analysis and from testing the components and so on. A model of this kind is given by the 1975 Rasmussen Report, published by the Nuclear Regulatory Commission under the title, *Reactor Safety Study: An Assessment of Accident Risks in U.S. Commercial Nuclear Power Plants*. In this report the system of probabilistic risk analysis is used. If the risks in a very complex system are examined, the failure rates of various components can be empirically tested, but it may not be possible to test the whole system. That can be done only by analysis, which always involved some assumptions which cannot be fully tested empirically about the statistical independence of a sequence of events. Probabilistic risk assessment always involves some incompletely testable assumptions regarding the statistical independence of individual events in a sequence leading to catastrophe. If they are truly independent, then the probabilities simply compound by multiplication and are usually very small because several events in sequence are involved in an accident with consequence. You can estimate with considerable confidence the probabilities of individual events from field experience with the failure of components (including the effects of human error), but the possibility of coincidental failures that are causally related can only be estimated through the exercise of imagination and perhaps some partial experimental testing. But you have to imagine the possible event first before you can test it, and that is why PRA can never be completely "scientific."

The expectation that policy analysis will influence policy is a somewhat different problem. People do not like to be told that their policies will not work. The problem is that the prediction of outcomes is only probabilistic and therefore much slippage can occur. The most probable outcome may not be the one that actually occurs. You can seldom prove that a policy just will not work. You can only say that it seems more plausible than not that it will not work or *vice versa*. Maybe the lament of social scientists about policy is justified.

Martin Greenberger's book, *Caught Unawares*, provides one of the best analyses I know of the effects of energy policy analysis on energy policy. One of the conclusions that Greenberger reaches is that the political effectiveness of energy reports was inversely proportional to their scholarly quality as judged by scholars. He considered the report of the highest quality to be one of several that were financed by the Ford Foundation, chaired by Hans Landsberg, and published in 1979 as *Energy, The Next Twenty Years*. It came out the same year as the CONAES study, *Energy in Transition, 1985–2010, Final Report of the Committee on Nuclear and Alternative Energy Systems*. The Ford Foundation report was given an extremely high rating by experts, but it sank without a trace almost immediately. One of the most influential reports, or temporarily influential reports, issued at the same time was the one by Stobaugh, Yergin and some other people in the Harvard Business School, which had an enormous impact and was given a low rating by scholars in Greenberger's survey. The CONAES Study was somewhere in between.

It is difficult, therefore, to give a simple answer to your question. It certainly seems to be the case that the influence of a report depends on the style and manner of presentation. In a certain sense, the more hard evidence is required to support a policy question, the more difficult it is to present a policy conclusion in a way that is accessible to a large number of people. And that is certainly a major part of the problem.

IBC: Is part of the problem related to your analogy of physics and engineering? What you say is that it is the engineering that sets the goal, not the physics. It is perhaps an idle hope to expect that the people who determine political or social policy will turn to social scientists at the stage of deciding on the goals rather than for implementation of a policy to achieve those goals.

HB: Well, perestroika would never have resulted from social science analysis. Although analysis may help to decide whether a policy design – *after* somebody has thought it up – will achieve the goals it aims at, there is a synthetic and imaginative quality to policy that is simply unrelated to analysis. The design of a policy is the result of an imaginative product, more like a work of art or the product of a craftsman, even though there is a component of analysis. There is a very high element of craft in policy design, no matter what field of policy one is discussing. And much of the debate is about how to relate the craft to the analysis. Surely analysis ought to be able to help evaluate the policies that are crafted, but there is always the role of imagination in policy design which simply does not necessarily follow directly out of the analysis. I am afraid that I have not given you a very good answer to your question.

IBC: There is no easy answer. That itself is a very interesting conclusion.

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